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THE MONIST

GOTTFRIED WILHELM LEIBNIZ.

(1646-1716.)

THIS number of *The Monist* is devoted to a commemoration of the scientific and philosophical work of Leibniz and its influences on modern thought. It is just two hundred years since Leibniz died, and thus it is fitting, as well as useful, that we should all remember just now rather particularly the mortal Leibniz and his undying work. The articles here outlined for this and the following issue of *The Monist* have been collected and prepared under the editorship of Mr. P. E. B. Jourdain, an English scholar well known to *Monist* readers through his many recent contributions on the subjects of physics and logic.

The first article is an account of Leibniz's life and work by C. Delisle Burns, and it gives a view of the various activities of Leibniz which are of general interest, and particularly the great part he took in the founding of academies. A description of Leibniz's logic by Philip E. B. Jourdain then follows. It has become more and more recognized of late years that logic was at the foundation of both Leibniz's mathematics and his metaphysics, and we have a most instructive example of the intimate connection of his logical and mathematical ideas when we study Leibniz's early mathematical manuscripts, which were published long after his death and are here translated by J. M. Child for the first time. Another article of

importance in connection with Leibniz's mathematics is Prof. Florian Cajori's account of his binary system of numeration that he held in great affection as leading to an arithmetic which was an "image of creation."

The influence of Descartes on Leibniz's philosophy is studied by C. Delisle Burns, and the influences that formed Leibniz's monadism are dealt with by T. Stearns Eliot. The author last mentioned also writes on the analogy between Leibniz's monads and the "finite centers" of Bradley's monism.

The last article brings us to Leibniz's modern influences. The logical influence of Leibniz on Lambert and later writers is touched upon in the above article on Leibniz's logic. It is seen also in a study of Bolzano by Miss Dorothy Maud Wrinch which will follow in the same connection in the January number of *The Monist*. It is conspicuous in the modern work of Frege, and of Peano, Russell and Couturat. It must be remembered of course, that the splendid work of Frege, which was almost wholly unaffected by any other logician but Leibniz, has combined with the work of Peano to influence the modern school of mathematical logicians.

A realization was given to part of Leibniz's ideal by Hermann Grassmann. It was intended that this number of *The Monist* should also celebrate the seventieth anniversary of Grassmann's prize for an essay on the connection of his geometrical analysis with Leibniz's Characteristic, which was awarded in 1846 by the Jablonowski Society of Leipsic. But this must be deferred until January. Then we shall present three articles by A. E. Heath. The first will be a critical sketch of the life and work of one whose writings contain the germ of many modern developments in mathematics and mathematical physics. Grassmann shared with Thomas Young the distinction of winning fame in both philology and mathematics. His biog-

raphy shows him as a homely and lovable man of wide interests, possessing to the last indomitable energy and unshaken faith in the power of his work. In the second article an analysis will be made of the factors which were and are at the root of the neglect of the work not only of Grassmann but also of all writers on the abstract questions of a basic science of form. The third article will show how Grassmann, starting from the geometrical Characteristic of Leibniz, applied the principles of his work previously published in 1844 to the realization of a true geometrical analysis. The author claims that in this analysis we have a complete fulfilment of the high hopes of Leibniz, and shows the relation of their work to modern non-metrical geometries and to symbolic analysis.

Portraits of Leibniz, Lambert, Bolzano, Grassmann, Frege, Peano and Russell, and some details about these portraits, will appear in the current (October) number of *The Open Court.*

The following gives the books most frequently cited in this number together with the abbreviations used throughout.

BIBLIOGRAPHY.

ABBREVIATIONS

Cantor: Moritz Cantor, *Vorlesungen über die Geschichte der Mathematik.* Vol. II, 2d ed., Leipsic, 1900; Vol. III, 2d. ed., Leipsic, 1901.

Couturat, 1901: Louis Couturat, *La Logique de Leibniz d'après des documents inédits.* Paris, 1901.

Couturat, 1903: Louis Couturat, *Opuscules et fragments inédits de Leibniz.* Paris, 1903.

On the nature and object of Russell's and Couturat's work on Leibniz, see Russell, pp. v-viii, 2-5, and *Mind,* N. S., Vol. XII, 1903, pp. 177-201.

Couturat made a profound study of Leibniz's published works and arrived independently at the same conclusion as Russell: that Leibniz's Metaphysics rests solely on the principles of his Logic. After this he extracted (and published in 1903) some of the most

interesting manuscripts of Leibniz preserved in the Royal Library of Hanover; and had in consequence to rewrite a large part of the book of 1901, but he did not have to modify his plan nor even to correct his chronological conjectures (Couturat, 1901, pp. x-xiv).

De Morgan's *Newton*: Augustus De Morgan, *Essays on the Life and Work of Newton.* Edited with Notes and Appendices by Philip E. B. Jourdain. Chicago and London, 1914.

G: C[arl] I[manuel] Gerhardt (Ed.), *Die philosophischen Schriften von G. W. Leibniz.* Berlin, 1875-1890.

G., 1846: C. I. Gerhardt (Ed.), *Historia et Origo Calculi Differentialis a G. G. Leibnitio conscripta. Zur zweiten Säcularfeier des Leibnizischen Geburtstages aus den Handschriften der Königlichen Bibliothek zu Hannover.* Hanover, 1846.

G., 1848: C. I. Gerhardt, *Die Entdeckung der Differentialrechnung durch Leibniz, mit Benutzung der Leibnizischen Manuscripte auf der Königlichen Bibliothek zu Hannover.* Halle, 1848.

G., 1855: C. I. Gerhardt, *Die Geschichte der höheren Analysis. Erste Abtheilung* [the only one which appeared]: *Die Entdeckung der höheren Analysis.* Halle, 1855.

G. *Bw.*: C. I. Gerhardt (Ed.), *Der Briefwechsel von Gottfried Wilhelm Leibniz mit Mathematikern.* Vol. I, Berlin, 1899. Cf. De Morgan's *Newton,* p. 106.

G. *math.*: C. I. Gerhardt (Ed.), *Leibnizens mathematische Schriften.* Berlin and Halle, 1849-1863. See De Morgan's *Newton,* pp. 71-72.

Guhrauer: G. E. Guhrauer, *Gottfried Wilhelm Freiherr von Leibnitz: Eine Biographie.* 2 vols. Breslau, 1846.

Klopp: Onno Klopp (Ed.), *Die Werke von Leibniz.* Hanover, 1864-1877.

Latta: Robert Latta (Tr.), *Leibniz:The Monadology and other Philosophical Writings.* Translated, with Introduction and Notes. Oxford, 1898.

Merz: John Theodore Merz, *Leibniz.* No. 8 of Blackwood's "Philosophical Classics for English Readers." Edinburgh and London, 1907.

Montgomery: George R. Montgomery, *Leibniz: Discourse on Metaphysics, Correspondence with Arnauld, and Monadology.* Chicago and London, 1902.

Rosenberger: Ferdinand Rosenberger, *Isaac Newton und seine physikalischen Principien. Ein Hauptstück aus der Entwickelungsgeschichte der modernen Physik.* Leipsic, 1895.

In Rosenberger's book, the passages which are relevant to Leibniz's work are as follows: Leibniz's mathematical correspondence with Oldenburg from 1674 (series for area of circle), Collins, and Newton (pp. 439-448); a short note on Leibniz's manuscripts (p. 447); Leibniz's publications of 1684 and 1686 (pp. 448-450); the progress of the calculus in the hands of Leibniz, the Bernoullis, and others (pp. 455-460); and the events which led up to the controversy and the controversy itself (pp. 460-506). Besides this, Leibniz's physical views, and so on, are mentioned on pp. 231-234, 239-247, 411-412, 512, 514-520.

Russell: Bertrand Russell, *A Critical Exposition of the Philosophy of Leibniz, with an Appendix of Leading Passages.* Cambridge, 1900.

Sorley: W. R. Sorley, "Leibnitz", *Encyclopaedia Britannica*, 9th ed., Vol. XIV, pp. 417-423. Edinburgh, 1882.

The same writer's article on Leibniz in the latest (11th) ed. of this *Encyclopædia* (Vol. XVI, Cambridge, 1911, pp. 385-390) is almost a reproduction of the above article: the body of the article has been somewhat condensed and the Bibliography at the end expanded.

Fren: A. Trendelenburg, *Historische Beiträge zur Philosophie.* 3 vols. Berlin, 1867.

U: Friedrich Ueberweg, *System der Logik und Geschichte der logischen Lehren.* 3d ed. Bonn, 1868.

LEIBNIZ'S LIFE AND WORK.

GOTTFRIED WILHELM LEIBNIZ was born on June 21, 1646, at Leipsic. His father and mother both belonged to what we may call the learned classes, and the Leibniz family had been known for some generations. The father of the philosopher was a notary and a professor of philosophy in the University of Leipsic. He had been married three times, Gottfried Wilhelm, born when his father was forty-nine, being the only son of his third wife. When Leibniz was six years old his father died, and his education during his school years was directed by his mother. In his autobiographical memoir he mentions the various obscure studies in which he seems to have delighted at an early age. He entered the University of Leipsic in 1661 as a student of law, having already read much in the classics and in scholastic philosophy. The title of his dissertation for the bachelor's degree (1663), *De principio individui,* marks his connection with the thought of Ockham and Nicholas de Cusa. He was apparently also affected by Raimundus Lullus, in his conception of symbolic logic and calculation. Owing to the officialism of those who granted degrees, Leibniz was unable to conclude his academic career at Leipsic and he therefore left his native Saxony never to return. Eventually he was made Doctor of Laws at Altorf near Nuremberg.

In this later period he seems to have come under the influence of Renaissance thought as it was in Bacon and

Hobbes, and he was affected by the mathematical conceptions of Descartes. His desire to know everything that he could led him to communicate with the Rosicrucians of Nuremberg, and in connection with them he dabbled in their form of chemistry which seems to have been a mixture of magic and learned jargon. But more important than this introduction to physical science was the meeting of Leibniz with the Baron von Boineburg, who had himself some interest in alchemy. The Baron induced Leibniz to leave Nuremberg with him for Frankfort, and there he was made a councillor of the supreme court of the Elector. From this time on Leibniz lived among courtiers and jurists.

It was at this period that he began his writings on jurisprudence, which he conceived should be systematized and made logical. He also began his philosophic writing with two tractates on motion, and at the request of his patron he brought out with an introduction an edition of a work by Marius Nizolius which is an attack, largely formal, upon the scholastics. The philosophical development of Leibniz will, however, be dealt with elsewhere, and here we shall confine attention to his more public activities.

The European situation at the end of the seventeenth century was unstable, owing in great part to the diplomatic device of the balance of power. Louis XIV loomed large, especially upon the German horizon and he appears to have been chiefly moved by that peculiar Renaissance myth—glory. After various pursuits of this intangible goal his activities so alarmed the Duke of Lorraine that in July 1670, the Duke attempted to form a league with the Electors of Mainz and Treves. It was suggested that England, Sweden and Holland should join the German states to prevent Louis from pursuing glory upon the banks of the Rhine. Leibniz was able to assist Boineburg in the nego-

tiations and he seems to have suggested a purely German league for defense against the military ambitions of France. It came to nothing. In the late summer, the duchy of Lorraine and the bishoprics were attacked and conquered by Louis, a beginning of evil still unended. Leibniz continued to urge the union of the German princes.

In 1672 he accompanied Boineburg to Paris, nominally upon private business of the Baron's, but in reality to attempt to turn the attention of the French king away from Germany and Holland. Leibniz had already worked out a scheme, which indeed had been suggested before, of an invasion of Egypt by Christian troops under the leadership of the French king. Glory, he conceived, might be there; and in any case Europe would be left in peace. The scheme was actually presented to and acknowledged by the foreign minister of Louis XIV, but nothing more was done in the matter. England and France attacked Holland—historians probably know why. At Paris, however, Leibniz continued, superintending the slow wits of the Baron's son and meeting various men of note and learning. At this time he seems first to have seriously studied mathematics and to have gone into the detail of the Cartesian philosophy.

From Paris he went with the Elector's ambassor for a short visit to London (January, 1673). The purpose of the embassy was to persuade Charles II to allow the interests of Germany to be considered in the treaty of peace with Holland. The request was refused, as it had been by Louis. But Leibniz took advantage of his visit to meet various learned men; and he was made a member of the Royal Society. We now hear for the first time of the work of Leibniz upon the higher mathematics. From 1675 to 1677 he was again in Paris and in 1676 completed his discovery of the differential calculus. Therein lay matter for controversy with Newton at a later date, but as it hardly

seems to be important which first made the discovery we may here avoid the issue. What is more interesting to remember is that Leibniz lived in London and Paris in the world of Christopher Wren and Robert Boyle, of Molière and Racine. There was a certain intellectual energy in the air which could not at that time be equaled anywhere else in the world.

In 1677 Leibniz left Paris. He had at one time thought of making his home there among the learned and the cultured, but an offer of a post in Hanover changed his plans. He visited London again for a week and then went on to Amsterdam and the Hague, where he met and conversed with Spinoza, and so to Hanover. For ten years he lived there as ducal librarian, and there he took up the task of collecting materials for a history of the house of Brunswick. But he was not to live retired. In the first place the European situation was again unsettled by the attack of Louis XIV upon Germany, in deliberate violation of a truce, on the obviously insincere plea that the Emperor was about to make peace with the Turks and might then turn his arms against France. The best defensive was known even then to be an offensive. The Revolution of 1688 in England gave new importance to the house of Hanover. Europe was thus already divided into Catholic and Protestant powers, which made utterly impossible the scheme of Leibniz and others for religious reunion.

From 1687 to 1691 Leibniz traveled to collect materials for his history in various parts of Germany and in Italy. He visited Venice, and at Rome was welcomed by various learned societies. There also he met learned Jesuits and heard of the missions in China, where he was given to understand there was much learning.[1] He paid a short visit to Naples and in 1689 reached Modena. But the new

[1] He suggests sarcastically in his letters that as the Europeans were sending missionaries to China to teach the truths of revelation, the Chinese should send missionaries to Europe to teach us the practice of natural religion.

stage in his life is marked chiefly by his connection with Berlin. He became what was practically a diplomatic agent there in 1700, and he wrote various political essays in support of Austria and of the making of Prussia into a kingdom. In Berlin also Leibniz met Christian Wolf, with whom he continued a correspondence from 1704 until his death, and who was recognized later as his philosophical successor.

We have an account of his personal appearance at about this date left by his secretary. He is said to have been a small man with broad shoulders and a slight stoop. His eyes were keen but small; his hair was originally dark but he had lost it all, and on his bald head there stood a bump the size of a pigeon's egg. It was, however, an age of wigs. His habits were ascetic. He slept little, and often in his chair without attempting to go to bed. He would go on with his reading even when suffering from an occasional illness. His emotional adventures were few, if at least we can judge from the fact that when he was fifty he proposed marriage to a lady who took time to consider it, whereupon Leibniz seized the opportunity to reconsider.

In public work the activity of Leibniz was of two kinds, diplomatic or juristic and academic. He conceived the idea of a logical jurisprudence, and his early attention seems to have been fixed upon the political situation. In 1659 he wrote an essay on the election of the king of the Poles, and in 1667 a *Nova methodus discendae docendaeque Jurisprudentiae*. His chief purpose, however, was exactness of definition and systematic treatment, and although he served in public life as a learned jurist and diplomat it is not in this sphere that he has contributed most.

Another public activity was his devotion to the religious reunion of Christendom. His attempts to reunite the Christian churches arose partly from his own training

and sentiments, partly no doubt from the fact that he was librarian at Hanover under the Catholic duke and under his successor the Protestant Ernst August. It was hardly a hundred years since the Reformation was established in the north and men of good will still shrank from taking it for granted that there must be divergence of religious forms and beliefs in Europe. Leibniz knew the scholastics and the best of the older Catholicism. He saw and appreciated the contemporary work of the Jesuits and he lived in the midst of a society very varied in its religion. Therefore he joined with enthusiasm those who hoped for some compromise between church officials and theologians of the old and the new schools. Most of his work was done by correspondence. On this subject he wrote to many Catholics, but the most important of his letters were addressed to Bossuet. The courtier bishop and theologian set out with great clearness the claims of the See of Rome. He said that Protestants were opinionated, that there was no evidence for Rome's ever having treated heretics as equals, and that the decrees of the Council of Trent could quite reasonably be accepted. Bossuet broke off the correspondence in 1694; but it was renewed and finally broken off by Leibniz in 1701. They could not agree, among other things, as to whether the Council of Trent should have introduced the Apocrypha into the Biblical canon.

Feeling ran fairly high even in the correspondence of scholars, although theological emotions had somewhat subsided since the days when the fathers of the Council of Trent pulled out each other's beards in an agony of excitement as to whether there was justification by faith only. Leibniz saw that the hope of any compromise grew less as each form of religion was more rigidly institutionalized, and doubtless those on the other side saw that the new churches lacked none of the assurance of the old. There was the added difficulty of political division more or less

corresponding to religious differences, and the German princes could hardly look with delight on the prospect of being catholicized by Louis XIV. So disagreement grew to discord and then to the silence which has divided for two hundred years the two great religious traditions of Europe.

Leibniz, however, was great enough to keep for himself some appreciation of what was best in the institution to which he dared not belong lest, as he said, it should stifle his thought. In a letter of 1691 he says, "You are right in regarding me as a Catholic at heart. I am one openly even, for it is only obstinacy that makes a heretic, and of this, thank God, my conscience does not accuse me. The essence of Catholicism consists not in external communion with the See of Rome...The true and essential communion which unites us to the body of Christ is love." The hopes for a religious reunion of Europe were based upon such sentiments as these, and although Leibniz was not ecclesiastically minded he might have done much for the future of Europe if this scheme had succeeded.

His public work in the conception and founding of academies was perhaps of more permanent and universal importance. To appreciate his position we must allow for the peculiarities of his age. In the first place there were ancient institutions representing the spiritual power of the Middle Ages at least on its intellectual side—the universities and the religious orders. The church at large could never have been the medium for intellectual progress, but it had within it a place for investigators, learned men and teachers. The universities still kept in Leibniz's day the form of the medieval *studia generalia*. They had been, however, for some years somewhat removed from the new currents of thought. They had become more and more formal in their view of learning, accepting the methods and matter of past knowledge and perpetuating them. In spite

of such brilliant accidents as Bacon and Hobbes or, in Leibniz's day, Newton, the universities were stiff with formulas. The religious orders in the Catholic countries were wealthy and their members had abundant leisure, but they had forgotten the possible connection of intelligence with religion. The older orders contained only commentators on the great scholastics, and the view taken of their duty to humanity was narrow and antiquated.

In Italy the custom had begun of cooperation between investigators, free from the traditions and the tutorial burdens—lightly borne indeed—of the universities. This is the origin of academies. They are the signs of the Renaissance, as universities are of the Middle Ages. They belong to the period of the humanists and polymaths and they lived on the appetite for new things which was only hampered by the mutual jealousy of their members. The Royal Society of London had been founded in 1660, the Paris Académie des Sciences in 1666; and it is with these two that Leibniz is chiefly connected. From his experience of their utility, he seems to have come to the conclusion that the idea of academies was valuable. Its importance for us here is largely historical, for academies have become, as universities had in Leibniz's day, opportunities for the mutual admiration of the obsolete. Their purpose, at least in the public mind, is rather to register the approval of established authorities than to give opportunity for new and fruitful departures from tradition. It is all the more important to recognize that they were once revolutionary intellectual associations, and it is as such that Leibniz looked to their principles as full of promise for the development of civilization.

Academies mark the new age in learning in two ways: In the first place an academy is a free association for investigation and the application of science to every-day needs and not for teaching or for explaining tradition.

This is one example of the mood of the Renaissance. The value set upon exceptional ability and the impulse to individual exploration in the intellectual as well as in the geographical world are here embodied. The famous Florentine Academy and the Roman Society which had an unfortunate notoriety under Paul II, were to their members, as they were to the public, associations of those who were willing and able to go beyond the known bounds of human knowledge. And the same spirit, less "pagan" on the one hand but more scientific on the other, was to be found in France and England during the late seventeenth century. The immense promise of the future gave the academies their best energy, and this promise could only be realized, it was felt, by individual or associated investigation into nature. Nothing could be more different from the spirit in which the universities had been founded: and in this spirit of progressive thought we have made but little advance upon the Renaissance enthusiasm.

In another sense the academies of Leibniz's day may be recognized as belonging to a stage of intellectual progress which has now been passed. We have seen that they are for the exceptional, by comparison to the universities. But on the other hand the Renaissance, even as late as the seventeenth century, was a period in which civilization depended upon a small clique in a world of uneducated and half brutalized "workers." Perhaps that world has not altogether disappeared. The position, however, of Descartes, Leibniz, and most scholars or scientists of the seventeenth century, could hardly be paralleled in our days. It is the position of courtiers, dependents and hangers-on of "great" men. Academies, indeed, still preserve the memory of their dependence upon favor as universities still preserve their old connection with the clergy. But we should be doing the activity of Leibniz an injustice if we did not allow for the limitations within which he worked.

The "reading public" was small, and the centers of civilization few. In addition to the London and Paris of his day we have a world-wide connection of great cities, and in place of his unwashed and semi-educated patrons we have vast numbers of men and women quite capable of appreciating a new scientific or literary idea. His achievement must, therefore, be measured by reference to the slender resources at his disposal, and we must imagine him rather a pioneer in the work of civilizing humanity than an exponent of all that may be done in that high task.

Leibniz was introduced to the Royal Society as a member in 1673; and he began his communications with the Paris Académie in 1675; though he could not become a member, as he was a Protestant. Both societies were looked upon as the very latest thing in learning and their members were often laughed at for their fantastic ideas. Swift's *Gulliver* and Butler's *Hudibras* contain the contemporary popular view of the practical applications of this new science.

Such was the institutional organization of learning. On the other hand, knowledge had vastly increased since the universities arose and was still increasing too quickly for the academies to assimilate or systematize it. We must, indeed, allow that there was much in the material valued by the academies which has eventually turned out worthless, although it is from what they collected that the most valuable part of our science arose. We must imagine a time when scholars spent as much time in devising a machine for making calculations as in elaborating the new mathematics. Out of such facts come the enthusiasm of Leibniz for organized learning. And this does not make him simply a passive agent of the vague needs of his time, for it required no little insight to grasp the situation and to suggest an advance.

The first need which appealed to Leibniz was that of systematization. He was himself, as we have seen, vastly learned, and he was also one of those few men whose reasoning had not been overcome by his learning. He was master of his "subjects," not they of him, and the much he had only gave him an appetite for more. But before his eyes there stretched the unlimited details of acquired learning then possessed by the scholars and the illimitable vistas of possible increase. He must have felt, first, like that librarian of Anatole France who pulled down upon himself his own catalog and died of it. And next, in the jungle of "facts" he felt himself helpless even to utilize what he knew was there. "We are poor," he writes, "in the midst of riches, and we are hampered by the excess of our resources." The primary need, therefore, was a system of the sciences. Of this there are two renderings in Leibniz, belonging to different stages in the development of his own conceptions. The former begins with theological and moral science and hardly includes what we should call physical science. The second plan gives theology and morality a much more restricted space and is chiefly concerned with what we should call science and history. This marks the change of emphasis in Leibniz's mind as he moved more and more towards mathematics and the new methods of thought.

During all this time Leibniz was attempting to establish some exact and universally valid symbolism or notation in philosophy such as was already established for algebra. This would be, as he continually says, a thread of Ariadne in the labyrinth of acquired knowledge. He hoped, as most men did then, for a geometrically exact philosophy. But we may put this aside for special treatment when the relations of Leibniz with Descartes are considered. What is important here is that before he

attacked the problem of academies he was planning an encyclopedia and a universal philosophic symbolism.

Along with plans for the systematization of knowledge went plans for the association of the learned. We have already seen that academies were a product of the age. Leibniz makes the following changes in the conceptions of their structure and purpose. First he is convinced that a society should be founded of an almost religious nature to promote for human good the cooperation of the learned and the thinkers; and, secondly, he looks forward to an international association of all those who love intellectual pursuits.

First, then, Leibniz proposed to the Royal Society of London to take up his idea of a cooperative encyclopedia of knowledge. There was no definite result. Leibniz had been affected by English influences,[2] and as late as 1680 he hoped that the Royal Society would act. "You will not find anywhere nowadays a better store of fine intelligence than I know of in England." So Leibniz writes. But not even compliments could make the work of the Royal Society really cooperative. Leibniz also tried the Académie at Paris with a like absence of result. He appealed to Louis XIV to found such a society as he was planning, and he hoped to persuade persons of power in the world to believe in the utility of knowledge. The only success he seems to have had was in that he contrived to make the Duke of Brunswick purchase in 1678 the secret of the making of phosphorus. Leibniz turned also to the learned and tried to persuade them to cooperate, independently of patronage. But whether because of mutual jealousy, an atavism not purged by learning, or because the majority could not see anything but their own subject, the learned

[2] Chiefly the *Plus Ultra* of Glanville (1636-1680) and the plan for a universal language by Wilkins.

were as irresponsive as the princes, and Leibniz's ideal society was never founded.

It is worth while for us, however, to remember his plan. He had been much impressed by the religious orders in Paris and especially by the Jesuits. They had riches and organization and they worked independently of local or national interests for the "eternal welfare" of men. Why should there not then be, said Leibniz, an order of the intelligent and learned, "in which besides religion the happiness of men in the present world should be arrived at?"[3] Such a society would be "philadelphic," and could not be founded except with some religious enthusiasm:[4] but it would have the devotedness and the organization of the Society of Jesus, without the rigidity of rule and the concentration upon authority. It would be an *Internationale des Savants*,—a spiritual power. Its members would preserve and increase our knowledge of the secrets of nature and they would study and publish knowledge of public utility.

The various appliances which might be invented are hinted at in Leibniz's attempt to make a machine for pumping the water out of mines, and another for controlling fire. He proposed the conservation of forests, the institution of a metric system of weights and measures, and various other practical reforms. His conception of the society of Wise Men is like that of Bacon's college in the New Atlantis. There was here the common Renaissance forecast of the elaborate machinery we have now at our disposal. But Leibniz perceived that unless an international society with humanitarian interest were devoted to this purpose, the growth of knowledge would be retarded and in its practical applications it would be enslaved to the prejudices and pettinesses of local lords or rival factions.

[3] Cf. letter quoted in Couturat, 1901, p. 507, note 3.

[4] "Societatis talis stabiliri nulla melius ratione posset quam religionis conspiratione."

And so indeed it has been. Those who know do not rule, but their knowledge is controlled by those whose only use for "science" is to attain more violently their primitive purposes. Leibniz foresaw what we know, that explosives and engines of destruction are first sought and more easily made effective than contrivances for making labor lighter or life more pleasant. We still use the houses of his century but we have discarded its guns as unworthy of us. The spiritual power is still longed for by the French political theorists. The Internationale was never founded.

But Leibniz's ideals were not altogether without practical effect. He saw with regret that Germany was without any society such as the Royal Society or the Académie des Sciences. He therefore suggested an academy at Berlin, pointing out both the practical utility of such a society and the prestige it would give. For nine years he worked at making the authorities accept the idea, and the Berlin Academy was at last established in 1711 (Jan. 19). Leibniz's work was a direct evidence of the dependence of the civilization of one country upon the advance made in others. It was not simply as a rival that the Berlin Academy was brought into existence but in order that the progress initiated in France and England should be assisted in Germany.

He would have contrived the foundation of another such society at Dresden but for the war with Charles XII of Sweden. At Vienna, Leibniz tried from 1712 to 1714 to obtain the support of the Emperor for an academy. He even suggested the possibility of its depending, according to the English plan, upon the subscriptions of its members, with some slight subvention from the funds for hospitals, etc. Being a Protestant, he had to declare that he did not desire to be president of the proposed society; but no concession could buy off the suspicion and even the open hostility of the Jesuits, who were strong enough to prevent the

Academy of Vienna from being founded. Leibniz, however, continued for some years to reside in Vienna, and his influence at least brought some recognition for unecclesiastical learning. He was able also at this period to affect the new civilization of Russia.

Leibniz had met Czar Peter at Hanover in July, 1697. The Czar had come, practically in disguise, as a member of his own embassy, and he was evidently open to new ideas. In 1708 Leibniz suggested to him the formation of a scientific society in Russia; but the war with the Turks prevented any action being taken.[5] Leibniz, as usual, made a note of the subjects to which such a society, in view of its surroundings, could specially devote itself. He suggested that geography would be most naturally the chief task of a Russian society, considering the vast unknown upon which Russia bordered. Thus in his mind there was an intimate connection between the foundation of national academies and the special work of each for the general good of all men.

So far we have seen how Leibniz suggested a religious or humanitarian task to be adopted by established societies, and then urged with partial success the formation of different new local or national societies. But he had all along kept before him the ideal of an international union of men "of learning and of good will." Thus in May, 1696, he wrote to Placcius: "Nothing is more useful than the union of the learned in societies. It would be best that there should be one such universal society divided as it were into distinct colleges. For such is the connection between the different parts of knowledge that only by mutual friendliness and assistance can they be made to progress." And again in October, 1697, he wrote: "So long as something valuable is done, I do not care whether it be done in Germany or in France, for I desire the good of the whole

[5] A society was, however, founded in 1724 at St. Petersburg.

human race. I am not a lover of Greece or Rome but of man."[6]

It is sufficiently obvious, then, that Leibniz although an active supporter of scientific progress in different countries, was a convinced internationalist. He does not conceive the two attitudes to be inconsistent, since in every step forward made by separate nations he saw a promise of good for the whole human race. But events since Leibniz's day have gradually obscured the more comprehensive ideal, and the primitive jealousies of different racial groups have taken control of science and even of the resources of art. Progress has been more rapid in those applications of science which divide men from one another. History and literature have become in every nation an apologia or a panegyric of that nation. The current of events was directed not by the plan of idealists but by the appetite of princes. Leibniz himself was not unaffected. In 1707 he was sent on a secret mission to Charles XII of Sweden who was at that time pursuing glory in Auerstadt near Leipsic; and Leibniz's scholarship was used by the Emperor for political writing about the situation following the Peace of Utrecht.

During all this time Leibniz had been continuing his official work upon the chronicles of the house of Brunswick. He speaks of the *mille distractions* of his life, which kept him from philosophy, and he complains to his friends that "at a court nothing like philosophy is wanted or asked for."[7] He had, however, written the *Nouveaux Essais* in 1704, and in 1710 the *Theodicée*. From 1711 until 1714 he lived chiefly at Vienna and there, in about 1712, he was made an imperial privy councillor and a baron. The *Monadologie* was written in 1714, to be presented to Prince Eugene: and when Leibniz returned to Hanover in that

[6] "Je suis non pas φιλέλλην ou φιλορωμαῖος, mais φιλάνθρωπος."

[7] Letter to Placcius, 1695, "in aulis scis longe aliud quaeri atque exspectari."

year he found that the Elector had left, owing to the death of Queen Anne in England. Leibniz had hopes of following his patron to London, and had in fact thought some years before that he would find in London more congenial companionship than in Hanover. But the Elector, now king of England, told the philosopher to go on with his writing of the history of the house of Brunswick. Unfortunately Leibniz had expressed his opinion some time before that the customs of the English should not be interfered with by their king, and the Hanoverian ministers viewed his possible liberalism as a danger. A legend says that George I was proud of having a Leibniz in one of his dominions and a Newton in the other; at any rate he kept them apart.

A form of arthritis, from which Leibniz had suffered for some years and to which his sedentary habits contributed, became more acute in 1715. The history of the house of Brunswick was, however, prepared in that year for publication.

Leibniz died on November 14, 1716. At his deathbed no clergy attended as he had seldom or never been to church, and no one but his secretary followed his body to the grave. The court was aware of the little value set by George I upon a mere historian of his greatness. No notice was taken of his death, even by the learned, except that a decorative oration was pronounced upon him in the Académie des Sciences of Paris. Berlin and London made no sign.

The impression made by Leibniz on his contemporaries seems not to have been very great, or it may be that the unfortunate controversy with Newton prevented his being judged rightly by the English scholars who would perhaps best have appreciated his work. Upon his own countrymen the work of Leibniz made no impression for many years after his death. He lived among courtiers and de-

pended for his livelihood upon what could be spared by princes after their expenditure in the pursuit of warlike glory. He suffered for his security. The state in the years since the Middle Ages has taken credit to itself for supporting art and science, as the church in earlier times is supposed to have done. The evidence for each claim is equally lacking.

In general character Leibniz seems to have been pleasant and not striking. In intellectual interest he is the representative of the old tradition of omniscient humanists who intervened between medieval scholasticism and modern thought. Devoutly religious in the untheological sense, he endeavored always to keep hold of the tradition of those who believed in the goodness of God. He does not seem to have experienced the heights or the depths of emotion, although he greatly valued Plato. But it is no small credit to his genius that he was able to see so keenly into the nature of things through the elegancies and sentimental egoisms of court life. And his human sympathy was far-seeing and comprehensive.

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THE LOGICAL WORK OF LEIBNIZ.

WHEN Leibniz's work is studied as a whole, as some of his remarks clearly show us that it ought to be studied,[1] we can see that his philosophy and his mathematics were founded in his logic. Although many have noticed the close connection of Leibniz's notions of his infinitesimal calculus and his monads, for example,[2] it was reserved for modern investigation to trace the complete story, both by reconstruction of Leibniz's thought and by taking into account hitherto unpublished documents written by Leibniz himself. This being the case, it is difficult not to complain of the way in which Leibniz's works have been published. Thus, Gerhardt, the editor of the most modern and complete collection of Leibniz's works, separated these works into "philosophical" and "mathematical." And yet Leibniz himself, in a letter to de l'Hôpital of December 27, 1694, had said: "My metaphysics is wholly mathematical";[3] and to Malebranche in March, 1699, he had said that "mathematicians need to be philosophers just as much as philosophers need to be mathematicians."[4]

In this article an attempt will be made to give an idea of Leibniz's logical work and plans for logical work. Great use has been made of Couturat's splendid book of 1901 mentioned in the Bibliography given above, but on some important points Couturat's account is supplemented. For example, this is so in the account (§ II) of the early ap-

[1] Cf. Couturat, 1901, pp. vii-ix.
[2] Thus cf. Latta, pp. 74-86.
[3] G. *math.*, Vol. II, p. 258.
[4] G., Vol. I, p. 356.

pearance of Leibniz's doctrine that the principle of identity held a very fundamental place in logic; in the sections (§§ IV, V) on the influence which guided Leibniz to a study of mathematics and on his mathematical work down to about the end of 1676; in the account (§ X) of the principle of continuity and its later developments; and in numerous footnotes throughout the paper. It cannot be too strongly emphasized that only these supplements are here treated at length, and that a knowledge of Couturat's book is assumed,—merely a tolerably full account of its contents has been given in the sections devoted to it.

I.

In a philosophical essay which Leibniz wrote in later life, under the name of "Gulielmus Pacidius," he said that when in his tenth year the library of his father, who was then dead, was thrown open to him, he seemed to be guided by the "*Tolle, lege*" of a higher voice, so that his natural thirst for knowledge led him to study the ancients and imbibe their spirit. "I burned," said he, "to get sight of the ancients, most of them known to me only by name, Cicero, Seneca, Pliny, Herodotus, Xenophon, Plato, and the historical writers, and many church fathers, Latin and Greek"; and soon it was with him as with "men walking in the sun, whose faces are browned without their knowing it."

It was characteristic of him to find some good in all he read.[5] "Like Socrates," he said, "I am always ready to learn";[6] and it was the study and spirit just mentioned, says Merz,[7] that led him to aim at two things which seemed to him to be foreign to the writers of the day: in words to attain clearness, and in matter usefulness.[8] The first aim led him to the study of logic, and, before he reached the age of twelve[9] he plunged with delight into the study

[5] G., Vol. VII, p. 526; Latta, pp. 1-2; Russell, pp. 5-7.

[6] Latta, p. 17.

[7] Merz, p. 13.

[8] *In verbis claritas, in rebus usus.*

[9] Couturat, 1901, pp. 33-34.

of scholastic logic.[10] He wrote out criticisms and plans for reform, and confessed that in later life he found great pleasure in re-reading his rough drafts written at the age of fourteen. At this age the idea occurred to him that just as the "predicaments" or categories of Aristotle serve to classify simple terms (concepts) in the order in which they furnish the matter of propositions, complex terms (propositions) might be classified in the order in which they furnish the matter of syllogisms, or of deduction generally. Neither he nor—probably—his teachers knew that that is exactly what geometricians do when they arrange their theorems in the order in which they are deduced from one another. Thus it was the mathematical method which was Leibniz's logical ideal even before he knew it, and it is not surprising that later on he took it as model and guide and grew to regard logic as a "universal mathematics."

Leibniz continued[11] to meditate on his idea of a classification of judgments about which his teachers had given him no information that was to the point, and it seems to have been in his eighteenth year that he arrived at thinking that all truths can be deduced from a small number of simple truths by analysis of the notions which are contained in them, and that all ideas can be reduced by decomposition to a small number of primitive and indefinable ideas. Thus we would only have to enumerate completely these simple ideas and thus form an "alphabet of human thoughts," and then combine them together, to obtain successively all complex ideas by an infallible process. This idea was a great joy to Leibniz, and while as a student of law at Leipsic University he was writing a dissertation on the necessity of introducing philosophical principles and reasoning into matters of law, and maintaining that the ancient jurists had brought so much thought and knowledge to bear upon their

[10] A short and good summary of the classical or syllogistic logic is given at *ibid.*, pp. 443-456.

[11] *Ibid.*, pp. 34-35.

subject that the principal task which they left to their successors was the systematic arrangement of the matter which they had collected, he was composing his treatise *De arte combinatoria*.[12] In it he showed that one of the principal applications of the art of combinations is logic, and more particularly the logic of discovery as opposed to demonstrative or syllogistic logic. The fundamental problem of the logic of discovery is, Given a concept as subject or predicate, to find all the proportions in which it occurs. Now, a proposition is a combination of two terms, a subject and a predicate. Thus the problem reduces to the problem of combinations.

In the latter part of this dissertation, Leibniz used and criticized the ideas of his predecessors, Raymond Lulle and others;[13] and one of the first applications given of the art of combinations was to the determination of the number of moods of the categorical syllogism.[14] Here I will draw attention to a relevant extract from a rather important manuscript of Leibniz. It was not referred to by Couturat, but is translated as the second of Leibniz's manuscripts on the infinitesimal calculus given in another article in this number of *The Monist*.

II.

When speaking of his early logical studies, Leibniz said in his *Historia et Origo*:[15] "When still a boy, when studying logic, he perceived that the ultimate analysis of truths that depend on reason reduces to these two things: definitions and identical truths; and that they alone of essentials are primitive and indemonstrable. And when it was objected to him that identical truths are useless and nugatory,

[12] Couturat, 1901, pp. 35-36; cf. also Merz, pp. 17-18, 106-114; Cantor, Vol. III, pp. 41-45.

[13] Couturat, 1901, pp. 36-39.

[14] On this and on Leibniz's later work on the syllogism, see *ibid.*, pp. 2-32.

[15] Cf. G., 1846, p. 4.

he showed the contrary by illustrations. Among other illustrations he showed that the great axiom that the whole is greater than the part could be demonstrated by a syllogism whose major premise was a definition and whose minor premise was an identical proposition. For, if of two things one is equal to a part of the other, the former is called the *less* and the latter the *greater*; let this be taken as the definition. Now, if to this definition we add the identical and undemonstrable axiom that everything possessed of magnitude is equal to itself, or $A = A$, then we have the syllogism:

"Whatever is equal to a part of another is less than that other (by definition);

"But the part is equal to a part of the whole (namely to itself, by identity);

"Therefore the part is less than the whole, Q. E. D."[16]

In another draft of the *Historia et Origo,* Leibniz speaks more at length about these early logical studies:[17] "Hitherto, while still a student, he was striving to bring logic itself to a certitude equal to that of arithmetic. He had observed from the first figure it was possible that the second and third might be deduced, not by employing conversion (which indeed itself seemed to him to need proof) but by employing solely the principle of contradiction; moreover, that conversions themselves could be demonstrated by the aid of the second and third figures by employing identical propositions; and, lastly, conversion being now demonstrated, by its aid the fourth figure could also be demonstrated; and thus that it was more indirect than the former (figures). Also he wondered very much at the force of these *identical truths,* for they were commonly considered to be nugatory and useless.[18] But later he perceived that the whole of arithmetic and geometry arose from identical

[16] Cf. Couturat, 1901, pp. 204-205. See also below, p. 590.

[17] G., 1846, p. 26, note 17.

[18] Cf. Couturat, 1901, pp. 8-12.

truths; and that, in general, all truths that were indemonstrable, if depending on pure reasoning, were identical; and that these combined with definitions produce identical truths. He gave an elegant example of this analysis in a demonstration of the theorem that the whole is greater than its part."

To Couturat's words[19] that Leibniz was concerned with showing the utility of identical propositions in reasoning and with defending them against the reproaches of insignificance and sterility urged against them by the empirical logicians, we may add two things: First, Leibniz seems to have held from early days the opinion that the foundations of logic are definitions and identical axioms;[20] secondly, in the *Historia* just mentioned, he traces to an identity his earliest mathematical discoveries in the summaton of series.

III.

We will now return to Leibniz's application of the art of combinations to the logic of discovery.[21] On analyzing all concepts by defining them—that is to say, by reducing them to combinations of simpler concepts— we arrive at a certain number of absolutely simple and indefinable concepts, and these "terms of the first order" are denoted by some such signs as numerals. "Terms of the second order" are obtained by combining in pairs those of the first order; and so on for terms of higher orders. Leibniz represented a compound term by the (symbolic) product of the numbers corresponding to the simple terms.

Leibniz was at that time still a novice in mathematics, and that explains many of the imperfections of the dissertation on the art of combinations; but still this early work

[19] *Ibid.*, p. 12.

[20] G., Vol. V, p. 92; Russell, pp. 17-19, 169; Couturat, 1901, p. 203. Cf. also the analogous example quoted from Leibniz and criticized by Frege, *Die Grundlagen der Arithmetik,* Breslau, 1884, pp. 7-8; Couturat, 1901, pp. 203, 205-207.

[21] Couturat, 1901, pp. 39-50.

contains the germ of his whole logic, which was with him a life-long study. That Leibniz was then a novice in mathematics comes out in the fact that he did not at first imagine his logic as a sort of algebra, but, since he was probably influenced by contemporary schemes, as a universal language or script.[22] This he had mentioned in his dissertation of 1666, and he developed it in the following years, especially from 1671 onward.[23] His "rational script" was, he says, a most powerful instrument of reason, and that it would promote commerce between nations should be esteemed the least of its uses. The notations or "characters" of a "real characteristic" represents ideas immediately and not words for them; thus, Egyptian and Chinese hieroglyphics and the symbols used by the alchemists for denoting substances are "real characters," and so they can be read off in various tongues; and, further, the "rational language" is formed on philosophical principles and is a help to *reasoning*.

IV.

According to Gerhardt[24] and Couturat,[25] Leibniz was led by logical investigations to the study of mathematics. About[26] the middle of the seventeenth century the study of mathematics in the universities of Germany was in a very bad state; and it is possibly enough to mention that his teachers were Johann Kühn and Erhard Weigel at the universities of Leipsic and Jena respectively. Still, Weigel seems to have gained a certain respect from Leibniz, and to have influenced him.[27] However, the facts that Leibniz had entered into correspondence with such men as Otto von Guericke and the learned Jesuit Honoratus Fabri of

[22] *Ibid.*, pp. 51-80. [23] *Ibid.*, pp. 59-61. [24] G., 1848, p. 7; G., 1855, p. 53.
[25] *Op. cit.*, p. 279. This is of course based on Leibniz's own statements.
[26] For the rest of this section, cf. G., 1848, pp. 7-9; G., 1855, pp. 53-54.
[27] Cf. Latta, p. 3.

Rome, and had sent the two parts of his *Hypothesis physica nova* to the lately founded learned Societies at London and Paris, show that Leibniz's active spirit was by no means satisfied with the knowledge he obtained in his university career. Before 1671 he had to depend almost entirely on books which came by chance into his hands, and thus it was that he was only acquainted with the beginnings of mathematical science and was for the most part ignorant of the progress made by the French, British and Italians during the seventeenth century. Also we must remember that he then considered law and history as his life-studies and thus only studied mathematics rather by the way and without any special industry. However these studies were very important for Leibniz, for he always kept in view their connection with logical researches and thus obtained exercise in expressing concepts by general signs. His first mathematical and philosophical writing of 1666 bears this character, and Leibniz himself repeatedly referred to it in the controversy about the discovery of the calculus.

In a letter[28] written from Mainz in the autumn of 1671 to the Duke of Brunswick-Lüneburg Leibniz announced a list of discoveries and plans for discoveries, arrived at by means of this new logical art, in natural science, mathematics, mechanics, optics, hydrostatics, pneumatics, and nautical science, not to speak of new ideas in law, theology and politics. Among these discoveries was that of a machine for performing more complicated operations than that of Pascal—multiplying, dividing, and extracting roots, as well as adding and subtracting.[29]

For Leibniz's mathematical education his stay in Paris, where he went in March of 1672 on a political mission, is

[28] Klopp, Vol. III, pp. 253ff.

[29] Sorley, p. 419. In G., 1848, p. 17; Latta, p. 6; and Merz, p. 53, it is implied that this machine was invented at Paris. This was also implied by Leibniz himself in § I of the article below on Leibniz's manuscripts relating to the infinitesimal calculus; but see Couturat, 1901, pp. 295-296. On the machine, see Cantor, Vol. III, p. 37.

of the greatest importance. Here for the first time he came into contact with the most eminent men of science of the time, and especially with Huygens who had presided over the French Academy since the year 1666. When Huygens published his celebrated *Horologium oscillatorium,* he sent a copy to Leibniz as a present. Leibniz saw from this work how very ignorant he was of mathematics, and his ambition to excel in this science flared up. In scientific conversations with Huygens the properties of numbers came into discussion, and Huygens, perhaps to test the talent of his new pupil, proposed to him the problem of finding the sum of a decreasing series of fractions whose numerators are unity and whose denominators are the triangular numbers. Leibniz found the correct result.[30]

Leibniz's intercourse with Huygens was interrupted by a journey to London in January of 1673.[31] In London, just as in Paris, he sought out the acquaintanceship of the celebrated men of England who lived in the capital. He had been in correspondence since 1670 with Henry Oldenburg, the secretary of the Royal Society, and met the mathematician Pell at the house of the chemist Robert Boyle. The conversation turned on the properties of numbers and Leibniz mentioned that he possessed a method of summing series of numbers by the help of their differences. When he explained himself more fully about this, Pell remarked that the method was contained in a book of Mouton called *De diametris apparentibus Solis et Lunae.* Leibniz had hitherto not known of this work; he borrowed it at once from Oldenburg, turned over its pages, and found that Mouton had obtained the same result in another way, and that his own method was more general.[32] By Pell Leibniz's attention was drawn to Mercator's *Logarithmotechnia,*

[30] G., 1848, pp. 17-19; G., 1855, p. 54.

[31] Cf. Cantor, Vol. III, p. 30.

[32] See the letter of Leibniz of February 3, 1673, to Oldenburg (G. *math.*, Vol. I, pp. 24ff).

especially because of the quadrature of the equilateral hyperbola contained in it, and Leibniz took this work with him to Paris. After his return to Paris he began, under Huygens's guidance, the study of the whole of higher mathematics. The *Géométrie* of Descartes, which hitherto he had known only superficially, the *Synopsis geometrica* of Honoratus Fabri, the writings of Gregory St. Vincent, and the letters of Pascal on the cycloid, were his guides.[33]

V.

We have another and rather different version of the way in which Leibniz was led to the study of mathematics. It was when he began to study at Leipsic University, which he entered in 1661—his fifteenth year—that he first became acquainted with the modern thinkers who had revolutionized science and philosophy.[34] "I remember," said Leibniz, "walking alone, at the age of fifteen, in a wood near Leipsic called the Rosenthal, to deliberate whether I should retain the doctrine of substantial forms. At last mechanism triumphed and induced me to apply myself to mathematics."[35]

In a letter of 1669 to Jacob Thomasius, one of his former teachers of philosophy at the University of Leipsic, Leibniz contended that the mechanical explanation of nature by magnitude, figure and motion alone is not inconsistent with the doctrines of Aristotle's Physics, in which he found more truth than in the Meditations of Descartes. Yet these qualities of bodies, he argued in 1668, require an incorporeal principle for their ultimate explanation. In 1671 he issued a *Hypothesis physica nova,* in which,

[33] G., 1848, pp. 19-20; G., 1855, pp. 54-55. On Leibniz's mathematical work of about this time, see Cantor, Vol. III, pp. 76-84, 115-118, 161-168, 179-184, 187-189, 191-216, 320-321; G., 1848, p. 15; G., 1855, pp. 33, 37-38, 48; G. 1846, p. xii, and the manuscripts on the calculus translated below; and Merz, pp. 50, 54-62. On the subsequent controversies to which this work gave rise, see Merz, pp. 84-96, 94-99, and Vol. III of Cantor.

[34] Latta, pp. 2-3.

[35] Cf. Merz, pp. 14-15; Latta, p. 3.

agreeing with Descartes that corporeal phenomena should be explained from motion, he contended that the original of this motion is a fine ether which constitutes light and, by penetrating all bodies in the direction of the earth's axis, produces the phenomena of gravity, elasticity and so on. The first part of the essay on concrete motion was dedicated to the Royal Society of London; the second part, on abstract motion, to the French Academy.[36]

VI.

It was in 1676 that Leibniz[37] seems first to have dreamed of a language which should at the same time be a calculus or algebra of thought, and then he definitely borrowed from mathematics his logical ideal.

But he soon found[38] that the construction *a priori* of a "rational language" was not so simple as he had believed at first, and in 1678 set about a comparative study of living languages for the purpose of extracting and combining the simple ideas expressed in them and of founding a "rational grammar";[39] and this language was by no means to be a calculus.[40]

Leibniz's problems then were, first, to make an inventory of human knowledge in which all known truths were to be demonstrated by reducing them to simple and evident principles, and, secondly, to invent signs to express the primitive concepts and their combinations and relations.[41] The second part was called the problem of the "Universal Characteristic"[42]—the characters being both what he called "real" and useful for reasoning, like the signs of arithmetic and algebra,—and the first part that of the "demonstrative encyclopedia."[43]

[36] Sorley, p. 419. On Leibniz's view of nature as a mechanism and his philosophy, cf. also Merz, pp. 41-43, 67-68, 72-73, 137-190.

[37] Couturat, 1901, pp. 61-62.

[38] *Ibid.*, pp. 63-64.

[39] *Ibid.*, pp. 64-79.

[40] *Ibid.*, pp. 78-79.

[41] *Ibid.*, pp. 79-80.

[42] *Ibid.*, pp. 81-118.

[43] *Ibid.*, pp. 119-175.

It was Leibniz who seems to have been the first to point out explicitly that "a part of the secret of analysis consists in the characteristic, that is to say, in the art of making a good use of one's notations,"[44] and we know,[45] both from his great step in inventing a supremely good notation and calculus for differentials and integrals and from the way in which he spoke of it from the very first, that he had the philosopher's property of being conscious of the help given to analysis by the invention of a *calculus of mathematical operations*— not "quantities"—which was very analogous to the calculus of ordinary algebra. The accusations that Leibniz had stolen ideas for an infinitesimal method are not only mistaken but also irrelevant. Leibniz himself said, without much exaggeration, that all his mathematical discoveries arose merely from the fact that he succeeded in finding symbols which appropriately expressed quantities and their relations.[46] In this connection we may mention that from Leibniz's Characteristic proceeded, besides his infinitesimal calculus and his dyadic arithmetic,[47] the use of a certain numerical notation in algebra and especially in the solution of simultaneous algebraic equations, the analogy between the development of, say, a binomial expansion and the repeated differentiation of a product of two factors, so that integration may be regarded as the operation of differentiation with a negative exponent, and so on.[48]

VII.

Leibniz formulated the conditions of a good Characteristic,[49] and clearly realized that it forms the basis for an

[44] Letter of 1693; Couturat, 1901, p. 83.
[45] Cf. *ibid.*, pp. 83-87.
[46] G. *math.*, Vol. VII, p. 17; Couturat, 1901, p. 84; Russell, p. 283.
[47] This is considered in another article in the present number.
[48] Couturat, 1901, pp. 473-500; Cantor, Vol. III, pp. 110-112, 230.
[49] Couturat, 1901, pp. 87-89.

algebra of logic, a *calculus ratiocinator* in which the rules of reasoning are translated by laws like those of algebra, and reasoning becomes a machinelike calculating process which frees the imagination where its action is not essential and thus increases the power of the mind.[50] With this tendency to economy of thought we may, it would seem, connect the opinion which Leibniz held on the value of the reduction of geometrical reasoning to analysis. "What is best and most convenient," said he,[51] "about my new (infinitesimal) calculus is that it offers truths by a kind of analysis and without any effort of imagination, which often only succeeds by chance, and that it gives us over Archimedes all the advantages which Vieta and Descartes had given us over Apollonius."

VIII.

The elaboration of the encyclopedia presupposed the knowledge of a universal method which should be applicable to all sciences, a "general science."[52] Little by little, the great plan for the encyclopedia, which occupied Leibniz at intervals from his twentieth year up to the time of his death, gave place gradually to the more restricted project of "beginnings of the general science," in which Leibniz would have exposed the principle of his method, that is to say his whole logic—which was an art of discovering as well as one of judging and demonstrating. All deduction, so Leibniz contended, rests on definitions, identical propositions; and so all truths can be demonstrated except identical and empirical propositions.[53] A definition is "nominal" when it indicates certain distinctive characters of the thing defined, so as to permit us to distinguish it from any other;

[50] *Ibid.*, pp. 96-103. Cf. on this point Jourdain, *Quart. Journ. Math.*, Vol. XLI, pp. 324-325, 329-332. Cf. also Russell, pp. 170, 206-208, 283-284.

[51] G. *math.*, Vol. II, p. 104; Russell, p. 283.

[52] Couturat, 1901, pp. 176-282. Cf. Latta, pp. 206-207.

[53] Couturat, 1901, pp. 184-188.

but a definition is only "real" when it shows the possibility or the existence of the thing. Indeed, the geometrical method requires that we demonstrate the possibility or ideal existence of every one of the figures defined either by indicating its construction or otherwise, so that every definition implies a theorem.[54]

Since the thorough analysis of truths and notions is the ideal of science, it is important to demonstrate the axioms, that is to say, to reduce them to definitions and identical propositions.[55] Indeed, every truth, whether necessary or contingent, is a relation of logical inclusion which can be discovered by simple analysis of the terms.[56]

Another part of Leibniz's logic is formed by questions arising out of the calculus of probabilities: the logic of probabilities is the science of temporal and contingent truths, and was, for Leibniz, a natural complement of the logic of certitude. And with this are connected considerations on the method of the natural sciences and the art of discovery.[57]

This art of discovery was regarded by Leibniz as his greatest discovery. He had cultivated it from his youth; it was to penetrate its secrets that he studied mathematics, because the sciences grouped together under that name were then the only ones in which this art was known and applied; and it was by trying to perfect it that he made all his mathematical discoveries. Thus we see why Leibniz's logic, mathematics, and philosophy were so closely connected, and also why Leibniz tried to give to philosophy a mathematical form.[58] But to extend the mathematical method to all sciences, the very idea of mathematics must be generalized, and this generalization resulted in the "Uni-

[54] *Ibid.*, pp. 188-195. On this theory of definitions and Leibniz's classification of ideas, see *ibid.*, pp. 195-200.

[55] *Ibid.*, pp. 200-207.

[56] *Ibid.*, pp. 208-213. On other principles (sufficient reason, and so on), see *ibid.*, pp. 213-239.

[57] *Ibid.*, pp. 239-278.

[58] *Ibid.*, pp. 278-282.

versal Mathematics,"[59] whence arose a general logic of relations.[60] But the only algebra which Leibniz developed at all was what may be called attempts at a "logical calculus," dealing with the relations of identity and inclusion,[61] and the "geometrical calculus," dealing with the direct study of figures and spatial relations.[62] Both are particular applications of the Characteristic, and both are essays in Universal Mathematics.

We know now,[63] from Leibniz's manuscripts, that he possessed almost all the principles of the logic of Boole and Schröder, and on certain points he was further advanced than Boole. The chief reason why Boole succeeded where Leibniz failed is that Boole made the calculus of logic rest on the exclusive consideration of extension—and not intension—of concepts.

In criticism of the main points of Leibniz's logic Couturat[64] has advanced the following remarks. The postulates of Leibniz's logic are two in number: (1) All our ideas are compounded out of a small number of simple ideas; (2) Complex ideas proceed from these simple ideas by uniform and symbolical combination analogous to arithmetical multiplication. With regard to (1), the number of simple ideas is very much greater than Leibniz believed. With regard to (2), logical "multiplication" is not the only operation of which concepts are susceptible: we have to consider also logical "addition" and "negation." Leibniz, because he did not take account of negation, could not explain how simple ideas—which are all compatible with one another—can generate, by combination, mutually contradictory or exclusive complex ideas. Further, even if Leib-

[59] *Ibid.*, pp. 283-322. [60] *Ibid.*, pp. 300-318.

[61] *Ibid.*, pp. 323-387. These attempts began in 1679.

[62] *Ibid.*, pp. 388-430; Cantor, Vol. III, pp. 33-36; cf. also Couturat, 1901, pp. 529-538. A special article by Mr. A. E. Heath on the relation of Grassmann's ideas to Leibniz's will appear in the January issue.

[63] *Ibid.*, pp. 386-387. [64] *Ibid.*, pp. 431-441.

niz had succeeded in building up an algebra of classical logic, the logic of relations would still have remained outside. Leibniz was conscious of this and with him are to be found the first attempts at such a logic, but he did not go far, owing, it would seem, to an excessive respect for the authority of Aristotle.

We must always remember that, in his *Nouveaux essais,* Leibniz[65] laid stress on the importance of the invention of the form of syllogisms, and remarked that it is "a kind of universal mathematics whose importance is not sufficiently known"; and that he also remarked[66] that there are good asyllogistic conclusions, such as "Jesus Christ is God, therefore the mother of Jesus Christ is the mother of God," and "if David is the father of Solomon, without doubt Solomon is the son of David."

X.

We will now consider Leibniz's "law of continuity" and its later fortunes.

Leibniz, in the course of his letter of 1687 to Pierre Bayle on a general principle useful in the explanation of the laws of nature[67] says: "It [the principle] is absolutely necessary in geometry, but it succeeds also in physics, because the sovereign wisdom, which is the source of all things, acts as a perfect geometer, following a harmony to which nothing can be added.... It may be enunciated thus: 'When the difference of two cases can be diminished below every given magnitude in the data or in what is posited, it must also be possible to diminish it below every given magnitude in what is sought or in what results'; or to speak more familiarly: 'When the cases (or what is given) continually approach and are finally merged in each

[65] G., Vol. V, p. 460; Russell, p. 282; U., p. 266; Couturat, 1901, p. 1.

[66] G., Vol. V, p. 461; Russell, p. 283.

[67] G., Vol. III, pp. 51-55; Russell, pp. 64, 222. Cf. Cantor, Vol. III, pp. 277-278, 367; G., Vol. IV, p. 229; Couturat, 1901, pp. 233-237; Latta, pp. 37-39, 71, 83-84, 376-377.

other, the consequences or events (or what is sought) must do so too.' Which depends again on a still more general principle, namely: 'When the data form a series, so do the consequences (*datis ordinatis etiam quaesita sunt ordinata*).' "

Later on Leibniz also expressed his "law of continuity" by saying that "nature never makes leaps,"[68] and it would certainly appear that each of the above forms of the law implies the other. We first find an exact treatment of the question with Bolzano, and this will be mentioned presently.

Couturat[69] remarked on the first form that the enunciation was quite mathematical and that the principle was evidently suggested to Leibniz by his work on the infinitesimal calculus, "of which the first postulate is that we have to do with functions that are *continuous* and have derivatives." However this may be, it is a fact that the phrase "a function is subject to the law of continuity" used to mean throughout the eighteenth century and the first few years of the nineteenth, that the function in question was not one of those which Euler maintained could appear in the integrals of partial differential equations and which are expressed by differential equations in different intervals.[70]

For the moment I will distinguish with Arbogast between the "contiguity" and "continuity" of a function—the word "continuous" being used in the sense of Euler and the word "contiguous" in the sense in which we now, after Bolzano and Cauchy, use the word "continuous," and which seems to be the sense in which Leibniz used the phrase "varying according to the law of continuity." The

[68] G., Vol. V, p. 49. Cf. the passages quoted in Russell, pp. 222-223, and the first of the grounds against extended atoms mentioned on p. 234. Cf also *ibid.*, pp. 63-66.

[69] Couturat, 1901, p. 235 note.

[70] Cf., for example, Jourdain, in *Isis*, Vol. I, 1914, pp. 669-700.

fact then seems to be that Leibniz and his immediate successors thought that every function which could appear in analysis, geometry, or mathematical physics, was continuous and therefore contiguous; Euler made it probable that some important functions were not continuous and some of these were contiguous and some not. Fourier showed convincingly that those functions which seemed discontinuous to Euler were really continuous, since they could be represented by trigonometrical series, and thus that discontinuity was no mar to continuity. Finally, in 1814, Cauchy freed the language of analysis from the difficulty that one and the same function could be both discontinuous and continuous according to the way in which it was represented, by ignoring the notion of continuity and keeping only that of contiguity. Cauchy, in 1814, spoke of contiguity as "continuity,"[71] and this will seem to us confusing only if we do not reflect that the name "continuous" could be used by another conception as its original bearer was deceased.

It is, by the way, somewhat remarkable that Fourier should, in spite of this discovery, have clung to Euler's idea of "continuity" of a function and should have left to Cauchy the formulation of that useful property of certain functions which we still, like Cauchy, call "continuity"; but such is the fact. Especially at the beginning of his career, Cauchy was greatly influenced by the work of Fourier, and we may describe a great part of Cauchy's work by saying that it was the precise description and introduction into pure mathematics of many of the new ideas to which Fourier was led. Though we see the germs of a new conception of the "continuity" of a function in a paper by Cauchy of 1814, the conception was precisely defined by him only in 1821, and it is to Bernard Bolzano—who seems to have been uninfluenced by Fourier and very much in-

[71] *Ibid.*, pp. 688, 689, 690.

fluenced by Leibniz—that the priority of a precise formulation of the new conception of the "continuity" of a function must be attributed.

In a paper published in 1817,[72] Bolzano criticized the statement that, because a function "varies according to the law of continuity," it must pass through all intermediate values before it can attain to a higher one, on two grounds. In the first place, this is a provable theorem,—if, as he seems tacitly to imply, the following "correct" definition of "continuity" is used. In the second place, in the above statement "an incorrect conception of *continuity* is taken as basis. According to a *correct explanation* of the conception of continuity, we understand by the phrase: 'a function $f(x)$ varies according to the law of continuity for all values of x which lie inside or outside certain limits,' only that, if x is any such value, the difference $f(x-\omega)-f(x)$ can be made smaller than any given magnitude if ω may be taken as small as we wish."

XI.

In somewhat close connection with the work of Leibniz on mathematical logic stands the work of Johann Heinrich Lambert,[73] who sought—not very successfully—to develop the logic of relations. Toward the middle of the nineteenth century, George Boole[74] independently worked out and published his famous calculus of logic, which is almost exactly what Leibniz would have called a *calculus ratiocinator*. At the same time as Boole, and independently of him or of anybody else, Augustus De Morgan began to work out logic as a calculus, and later on, taking as his guide the maxim that logic should not consider merely certain kinds of deduction but deduction quite generally,

[72] See the further account and references, *ibid.*, pp. 695-697.

[73] See the historical parts of John Venn's *Symbolic Logic*, London, 1881; 2d ed., 1894, quoted by Jourdain, *Quart. Journ. of Math.*, Vol. XLI, p. 332.

[74] Cf. Jourdain, *loc. cit.*, pp. 332-352.

founded all the essential parts of the logic of relations. William Stanley Jevons[75] criticized and popularized Boole's work; and Charles S. Peirce, Richard Dedekind,[76] Ernst Schröder, Hermann and Robert Grassmann, Hugh MacColl,[77] John Venn, and many others, either developed the work of Boole and De Morgan or built up systems of calculative logic in modes which were largely independent of the work of others.

But it was in the work of Gottlob Frege, Guiseppe Peano, Bertrand Russell, and Alfred North Whitehead, that we find a closer approach to the *lingua characteristica* dreamed of by Leibniz. To this work other articles in this number will be devoted.

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[75] Cf. Jourdain, *loc. cit.*, Vol. XLIV, pp. 113-128.
[76] Cf. *Monist* for July, 1916, pp. 415-427.
[77] Cf. Jourdain, *loc. cit.*, Vol. XLIII, pp. 219-236.

LEIBNIZ AND DESCARTES.

THE influence of Descartes appears in almost every detail of the philosophy of Leibniz. Scholasticism and historical studies were subordinated as Leibniz grew older, and even in the conception of activity in which he opposes Descartes, the argument is largely Cartesian. But we shall leave the implications of the two metaphysical systems to be dealt with in the discussion of Leibniz's theory of monads. Here we shall attempt to estimate only (1) the dependence of Leibniz upon Descartes for his conceptions of method, (2) his relation to Descartes in psychological questions, and (3) his dependence upon the Cartesian mechanism in physical science. In general Leibniz held that Cartesianism was "the anteroom of philosophy"; and although he criticizes Descartes more frequently than any other philosopher, the very frequency with which the name appears in Leibniz's works is a sign of the immense importance to him of the Cartesian philosophy. As to method, we may distinguish the general question of mathematical reasoning from the particular suggestion of Descartes as to philosophical doubt. This latter was made very prominent in the popular renderings of Descartes's philosophy; and it is the conception of methodic doubt which still rouses the anger of the survivors of Descartes's oldest opponents, the scholastics. For there are still in many parts of Europe schools of thought which are pre-Cartesian, and doubt to them has an ugly sound. Des-

cartes's doubt indeed proceeds from a knowledge of the fact that "there is no opinion however absurd or incredible which has not been maintained by some one of the philosophers."[1] A sort of relativism is generally supposed to be the result, as it is the result among many still when they first discover that their own beliefs and customs are not universal. To Descartes, however, the discovery seemed to show that every proposition must be doubted until some point was reached at which doubt was no longer possible. In the view of Leibniz this method was good, but the emphasis was in the wrong place. He says in a letter of 1696 to Bernoulli that "if Descartes, when he said that everything should be doubted, meant only what I propose, he was right; but in fact he erred in two ways, by doubting too much and by ceasing to doubt too readily." Leibniz wanted the emphasis to be laid on the desire for proof; that is to say, he corrected the method by making it a demand for reasons. And we can see how it would modify the effect of methodic doubt if it meant, not the rejection of any proposition which was not obvious, for obviousness itself may be difficult to distinguish, but the refusal to accept any statement without evidence. Again as regards ceasing to doubt, Leibniz pointed out the weakness in Descartes's conception of "clear ideas." This seemed to give no intrinsic criticism of what must be accepted and what not. He, therefore, suggested that the criterion was that the "idea" should, when analyzed, be seen to be not contradictory. Thus, as Leibniz says, the "idea" of a thousand sided figure is not, in the ordinary sense, a "clear one"; but it implies no contradiction in itself. The fundamental likeness between Leibniz and Descartes is in the conception that we can go back into experience until we come to unassailable or self-evident truths; and the manner in which these truths are conceived is alike in both,

[1] *Discours de la méthode*, Part II.

although Leibniz makes more clearly than Descartes the distinction between *vérités éternelles* (*a priori*) and *vérités de fait* (*a posteriori*).

In connection with this method and with the mechanism of Descartes, we can observe Leibniz's dependence in his estimate of mathematical symbolism. His scheme for a universal philosophical language appears to have been made out before he saw Descartes's letter on the subject. But there is no doubt of the source of the high value given by Leibniz to mathematics as a guide to philosophical method. It is in part the common thought of the age which had achieved so much by the application of exact mathematical reasoning to the data of physical science. The nature of things seemed to be disclosed when the master-key of calculation was used. "I believed," says Descartes, "that I could borrow all that was best both in geometrical analysis and in algebra and correct all the defects of one by the help of the other." Leibniz carries this conception further by arguing that we could make for philosophy a real symbolism (*caractéristique*), like the numbers in arithmetic or the signs in algebra. "If we had a symbolism," he says, "we should be able to reason in metaphysics and morals in much the same way as in geometry and analysis." And as mathematics has developed because of the signs we have invented, so philosophy would grow by the adoption of symbolic logic. What we need, he says, is not a vague statement concerning the limitations of reasoning, but an exact method. All reasoning is calculation, but, as against Descartes, it is not therefore mathematical. As far as men do think effectively in philosophy their thinking is "mechanical"; it is the primitive nature of the mechanism which is the source of the trouble. How happy would philosophers be if they adopted the universal symbolism; for then "when a dispute arose, it would suffice to take their pencils in their hands, to sit down to their slates

and to say to each other, with a friend as witness if they liked: Let us calculate."[2] This situation Leibniz imagined himself to have all but reached. "In the general characteristic or universal calculus," he says, "I have definitions, axioms and very remarkable theorems and problems in regard to coincidence, identity, similitude, relation, power or cause, and substance, and everywhere I advance with symbols in as precise and strict a manner as in algebra."[3] This is in the mood of the Cartesians who hoped to explain everything *more geometrico*.

In the second place, Leibniz's psychology is closely related to that of Descartes. We may omit the discussion of the epistemological criticism of Descartes's *Cogito, ergo sum*. It belongs to the general body of Leibniz's positive philosophy, and is important as connecting Descartes with Leibniz only in so far as Leibniz says that one's own existence is *not* a premise for necessary truths and is not any more certain than the existence of one's thoughts.[4] In psychological description Leibniz emphasizes the fact of unconscious perception, accepting more or less exactly the Cartesian idea of perception. Thus "perception" should be distinguished from apperception or consciousness. "In this matter the Cartesians have fallen into a serious error, in that they treat as non-existent those perceptions of which we are not conscious." Unconscious mental states are therefore added to the list of psychological facts; and their existence is used to show the nature of some monads. But in the main the Cartesian, as opposed to the scholastic, psychologically is accepted.

Thirdly, as to the use of mechanical conceptions for physical science, this was of course not peculiar to Descartes and Leibniz. It was the common ground of all

[2] G., Vol. VII, p. 200, quoted in Russell.
[3] Letter to Arnauld, Jan. 14, 1688. Cf. Montgomery, p. 241.
[4] Russell, § 102.

who made progress in the understanding of nature in the seventeenth century. Against the vagueness of the metaphysical physics inherited from the Middle Ages, it was effective not only upon grounds of general reasoning but also in the results it had to show. Leibniz, more even than Descartes, valued such results and in that he followed the ideals of Bacon but, naturally, with more intelligence. He felt that the bearing of scientific investigation upon the ordinary task of human life was not unimportant. The debt Leibniz owed to Descartes is acknowledged to have been great even before Leibniz came to Paris; but in Paris he seems to have taken up the new scientific and mathematical method with renewed energy. In the letters to Malebranche[5] he puts his position most clearly, and in one of 1679, which was apparently never sent, he writes: "It seems that all the harvest of Descartes's philosophy is over, or that the hope that was in it has perished in the bud with the death of its author; for the majority of Cartesians are only commentators." In the same letter he says of Descartes, "there are perhaps few who perceive as clearly as I the greatness of his mind," but "his geometry is what I think least valuable in Descartes." And in the letters to Arnauld he is continually correcting or criticizing the geometry of Descartes. The hope that there might be great practical results had been frustrated, and even the theoretical development seemed lacking. But there is no hesitation in Leibniz as to the value of the geometrical conceptions of science. It is true that these seemed to imply a complete removal of the "spiritual" and the "supernatural" from the regions dealt with in science, and old final causes would also disappear. Even this, however, although it was a reason for Leibniz's ultimate repudiation of Cartesian metaphysics, could not shake his belief in Cartesian physics.

[5] G., Vol. I, pp. 334f.

In the purely metaphysical issue Leibniz seems to grant to Descartes the arrangement of the machine of the universe by God; but he makes a small change, for "it is more reasonable and more worthy of God to suppose that he has created the machinery of the world in such a fashion from the very start that without doing violence at every moment to the two great laws of nature, that of force and that of direction, but rather by following them exactly (except in the case of miracles), it so comes about that the internal springs of bodies are ready to act of themselves, as they should, at the very moment when the soul has a conforming desire or thought."[6] The whole conception here is Cartesian—the machine and the springs of bodies and the desiring soul. The suggestion that what is "worthy of God" is true may perhaps be regarded as Leibnizian; but even that is in some part shared by Descartes, in succession to the established tradition according to which Aquinas long before could prove that Paradise was in the east, because the east is more "noble." At this point, however, Leibniz parts from Descartes and endeavors, still with an eye to Cartesian influences, to render experience not as mechanism with a parallel mentalism, but as activity and preestablished harmony. He was, in his own conception, restoring teleology to metaphysics and spirit to nature. But the result must be dealt with in the discussion of Leibniz's system as a whole. He follows Descartes at least so far as to begin with his description of facts.

The debt of Leibniz to Descartes is perhaps not less great in that metaphysical issue upon which they differ fundamentally—the conception of substance. It seems to be possible that the true source of concepts each had is not yet fully investigated by historians; but as the problem is now generally stated, Descartes stands for (*a*) extension as the nature of one kind of substance; and also, by implica-

[6] Letter to Arnauld, April 30, 1687.

tion, for (*b*) the real unity of all separate "things" in one (as in Spinoza's theory) or in two forms. Against this Leibniz stands for (*a*) activity or *actus,* in the scholastic sense, as the ultimate nature of all existents, and (*b*) individual units called monads, which are not in any sense less real than the whole within which they are related. The opposition does not involve a complete reversal of views, although Leibniz writes, "Extension is nothing but a certain indefinite repetition of things in so far as they are similar to each other and indiscernible. It presupposes things which are repeated." In such words he seems to imply that he had "reduced" extension to monads. Probably the source of both Descartes's and Leibniz's reasoning on matter is to be found in the theories of late scholasticism, sometimes called nominalism.

At this point we may perhaps note the relation of Leibniz to scholasticism, for it is not very different from that of Descartes, who had made less extensive study of its literature but was not for that reason any less affected by its leading conceptions. It is usual to consider scholasticism chiefly as a system of logic. The Aristotelian syllogism and the philosophical method, in part misleading, with which the name of Aristotle was connected, did undoubtedly color the whole of the medieval tradition in philosophy and science. This had been brought to an almost absurd elaboration by Lullus: and all this Leibniz acknowledges to have greatly impressed him. But this is not the most valuable part of scholasticism, nor is it the point in which scholasticism has most importance in the history of philosophy. For, first, we must recognize that scholasticism did not mean in the seventeenth century the theory of Aquinas and Scotus only, or chiefly, but the theory of Ockham. There had been a revival of Thomism, but in the main the philosophical tradition was such as Ockham had left it, and in the *matter* as opposed to the *method* the

new thought of the Renaissance depended upon what the histories of philosophy usually call nominalism or conceptualism.

The idea of *extension* as the nature of "substance" is to be found in Ockham. Thus "quantity" (used as meaning extension) is not distinct from "substance."[7] The point cannot be argued here; but in Ockham's eagerness to be rid of "quantity" as a real thing, he seems to have persuaded himself that the reality which most people call substance was quantity or extension. The influence on Descartes may have been very indirect or even unconscious. On the other hand Leibniz is generally recognized to have owed the conception of *actus* or activity to the scholastics. In his letters to the Jesuit Des Bosses this is abundantly clear. It is not, however, sufficiently noticed that this activity is conceived as individualized also because of the late scholastic tradition, for which again the name of Ockham may be taken to stand. The word "monad" may have been due to Giordano Bruno; but the phrase should not be forgotten by which Ockham revolutionized scholastic metaphysics: "Everything outside the mind is in itself individual in such a way that *itself* without any addition (e. g., the principle of individuation, etc.) is a 'this.' "[8] Leibniz's monadism is at least in part affected by this suggestion; but he does indeed often go back to the Thomistic influence when he "explains" individuation of the finite monad by some process of connection with "matter" or *potentia*. He might have maintained with the late scholasticism that the individual (*illud quod*) is the only substance and needs no further "explanation." But whatever the source of his thought, Leibniz clearly allowed for ultimately real individuals and he granted the calculability of phenomena in terms which imply all that Descartes intended to indicate by "extension." The rest of his doctrine was not Cartesian;

[7] *De sacramento altaris*, q. 3. [8] *In sententias*, q. VI.

but his continual attacks upon the idea that extension is the ultimate nature of matter should not blind us to the amount of general agreement between Descartes and himself, at least as opposed to the official and established conceptions of the day.

The relation between the two may perhaps be put in this way: For Descartes the calculability of phenomena is fundamental and there is nothing more to be said about it. The *nexus* between things is mechanical, in the sense that origination or spontaneity within the system may be left out of account in our description of the material universe. And this position has become so familiar to us that it is hardly valued by philosophers except for exercising their wits in discovering in what sense it may be mistaken. This position was known to Leibniz as the new doctrine which had overcome the "spiritualism" of the medieval metaphysics of nature. And undoubtedly he saw its complete validity for the description of phenomena, or the explanation of them in so far as that can be had by showing how they are connected. He disputed the details of the Cartesian geometry, but he granted the calculability of phenomena.

Descartes had, however, left upon his hands, so to speak, the kind of substance which was called soul or mind, thus creating a problem as to the connection of soul and body solved in one way by the occasionalists and in another by Spinoza. Leibniz attempts to avoid the difficulty by beginning with one type of ultimate reality, active substance. The meaning of this can only be rendered in a full account of Leibniz's philosophy. But even in Leibniz's monadism appear the automata and machines of Descartes. Thus he says,[9] "Every organic body of a living being is a kind of divine machine or natural automaton;" and again,[10] "Descartes saw that souls cannot impart force

[9] *Monadology*, 69. [10] Ibid., 80.

to bodies because there is always the same quantity of force in matter. Yet he thought that the soul could change the direction of bodies. This was, however, because at that time the law of nature which affirms also the conservation of the same total direction in the motion of matter, was not known. If he had known that law he would have come upon my system of preestablished harmony. According to this system bodies act as if (to suppose the impossible) there were not souls at all and souls act as if there were no bodies, and yet both body and soul act as if the one were influencing the other." There is much force in your "as if"! But in any case Leibniz grants that one may neglect soul in describing bodily changes, the correctness or incorrectness of which it is not our present purpose to discover. The important fact for us here is that it was only by retaining the Carestian doctrine as to natural phenomena being in some sense (whether fundamentally or superficially) calculable, that Leibniz was able to contribute to the progress of our knowledge of the universe. It is beside the point, in this regard, to ask what metaphysical truth is implied in the processes of physical science. That problem may be solved or left unsolved while the undeniable fact must be recognized that the Cartesian hypothesis has led to a control of natural forces and a power of prediction which can hardly be refused the name of knowledge.

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THE DEVELOPMENT OF LEIBNIZ'S MONADISM.

THE study of the *Monadology* may be comprised in three stages. In the first we isolate the work; with no other aid than the philosophical counters which itself employs, we attempt to draw its fantastic world around us and find it real. Perhaps we supplement it by searching in other works of Leibniz for elucidations of points which are not clear; but in any case we take the *Monadology* as a creed and test our possibilities of belief. No philosophy can be understood without this preliminary effort to accept it on its own terms; but its true value can never be extracted solely in that way. The perfected or the summarized form of any system is the starting point, not the terminus of study. We must effect a radical restatement, find in it motives and problems which are ours, giving it the dignity of a place in the history of science when we withdraw from it the sanctity of a religion. In losing the consistency of a closed system, it gains the consistency of reason, is attached to something larger than itself. Russell and Couturat have accomplished this revaluation for Leibniz. But beside the leading motive, the reason of a philosophy, there are other strata both below and above: prejudices, traditions, suggestions, motives which imperfectly assimilate to the central motive, all of which combine to give to the system the form which it has. The present essay is merely a preface to the investigation of these forces.

There are influences of suggestion, influences of tradition, personal influences, and, moreover, there is more than one conscious interest. Among influences of the first sort upon Leibniz (none of them of the highest importance)

I should class a variety of authors whose contributions to Leibniz are more verbal than profound. Leibniz's reading was wide beyond any point of selection, and he appears to have derived some entertainment from such philosophers as Giordano Bruno, Maimonides, and the Averrhoists.[1] Bruno is a classic example of influence in the most superficial sense. It is not certain, nor is it important, at what period Leibniz became acquainted with Bruno's works. For the probability that Leibniz was struck by the figurative language, that Bruno may have been in the background when Leibniz wrote some of his more imaginative passages, there is evidence enough. For the probability that Bruno affected Leibniz's thought there is no evidence whatever. What we have is a statement which bears strong superficial resemblances to the statement of Leibniz; the arguments, such as they are, the steps which lead up to the statement, are not similar. Leibniz's arguments are sufficiently strong not to demand support from the fact that there were monadologists before Leibniz. To his imagination we may concede plagiarism. But it is with the sources of his thought, not with the sources of his imagery, that we are concerned.

The other sources mentioned may be dismissed in the same way. It is interesting, perhaps, but not valuable, to observe that Leibniz read with appreciation a book by Maimonides. And though he never couples the names of Spinoza and Maimonides together, the notes which he made upon this book single out just the points of resemblance to the *Theologico-politicus*—the first work of Spinoza that he read. He was interested in Hebrew and Arabic studies. Bossuet sends to him for a translation of the Talmud. He announces to Bossuet a translation of the Koran. A dialogue of 1676 shows that he knew,

[1] For Bruno see H. Brunnhofer, *G. Brunos Lehre vom Kleinsten.* For Maimonides see Foucher de Careil: *Leibniz et la philosophie juive;* Rubin: *Erkenntnistheorie Maimons.*

through Maimonides, the doctrines of the Averrhoists and of a certain Jewish sect, the Motekallem. In 1687, while traveling in Bavaria, he undertook some study of the Kabbala, and perhaps noticed the theory of emanation from an infinite being which consists in an indivisible point—and the microcosm is said to be a familiar idea in Jewish philosophy. These studies, rather shallow it is true, illustrate Leibniz's insatiable curiosity toward every sort of theological hocus-pocus. Monadism was probably a satisfaction of this side of Leibniz's mind, as well as the outcome of his logical and metaphysical thought.

Of influences of suggestion there is only one which may have been of the first importance—the influence of Plato, to be treated later. The main influences which directed Leibniz are of three kinds: the scholastic Aristotelian tradition in which he was brought up, the very early stimulus of a personal teacher toward a mathematical conception of the universe, and Leibniz's temporary adhesion to atomism. His chief motives, more or less corresponding to this classification, were theological, logical and physical.

Merz expresses the conventional opinion[2] in saying that the *De principio individui* "bears witness to the young author's knowledge of scholastic learning as well as to his dexterity in handling their dialectic methods." Incompetent to impugn the scholastic erudition of young Leibniz, a perusal of this document impels me to exclaim with Kabitz, "as if the copious citation of passages from scholastic compendia proved any 'astonishing' learning on the part of Leibniz; as if he could not obtain these quotations just as well second-hand!"[3] The treatise is very short and very dull. Two or three passages in it are often quoted. "Pono igitur: omne individuum sua tota entitate individuatur"; and "Sed si omnis intellectus creatus tolleretur, illa

[2] Merz, p. 15.
[3] Kabitz, *Entstehung der Philosophie des jungen Leibniz*, p. 50.

relatio periret, et tamen res individuarentur, ergo tunc se ipsis." The principle of individuation is not mental, nor is it negative. Though Leibniz documents this work with such names as Occam, Scotus, Aquinas, Suarez, Molina, Zabarella, what the thesis shows is not extent of learning or originality of thought. It shows that there was a certain body of inheritance which pointed in a certain direction. It shows a scholastic point of view from which Leibniz never really escaped, and which he never wholly rejected.[4] In the light of these quotations is to be interpreted not only monadism, but the materialistic atomism which for a time engaged his attention. At this early period, and indeed throughout his life, there is little evidence of direct adaptations from Aristotle. But here as always one finds the acceptance of the problem of substance, transmitted from Aristotle through the form which the school had given it. In some ways diametrically in opposition with Aristotle, this scholastic view of substance which Leibniz held is yet an Aristotelian inheritance. This point is of capital importance.

It appears that Leibniz abandoned his study of the philosophers of the church when he felt called, at a very early age, to the mechanical view of nature (Merz, p. 15). But there was never a complete renunciation, and Leibniz, who seldom spoke ill of a dead philosopher, always praises the schoolmen. The change was a transition and not an apostasy. In 1663, at Jena, while pursuing his studies in jurisprudence, he fell under the influence of Weigel. Weigel was acquainted with the work of Copernicus, Kepler and Galileo. Kabitz says (*op. cit.*, p. 112) that "the fundamental conception of Leibniz's system (according to which the universe is an harmonic, mathematico-logical related whole .... became a firm conviction with Leibniz

[4] Nolen, *Quid L. Aristoteli debuerit*, p. 27: "mea doctrina de substantia composita videtur esse ipsa doctrina scholae peripateticae. Nisi quod ille monadas non agnovit."

through Weigel, before he was acquainted with the work of Hobbes." Bisterfeld of Leyden is another mathematician admired by Leibniz in his youth, and his influence is supposed to be visible in the *Ars combinatoria.* The idea of a harmony of a universe of individual substances is present in other writings of Leibniz's adolescence.

Leibniz's scholastic training in metaphysics under Thomasius was followed by that period in which, as he says, "having freed myself from the yoke of Aristotle [by which he means the attenuated scholasticism of his day], I took to the void and the atoms, for that is the view which best satisfies the imagination."[5] This may have been about 1666.[6] It is easy to see from the *De principio individui* (written, according to his own chronology, when he had already fallen under the influence of Gassendi) that this liberation was merely a development of extreme nominalism in the currents of his time. In 1676 he can still write, "Ego magis magisque persuasus sum de corporibus insecabilibus. . . . simplicissima esse debent ac proinde sphaerica," but goes on to say "Nullus enim locus est tam parvus quin fingi possit esse in eo sphaeram ipso minorem. Ponamus hoc ita esse, nullus erit locus assignabilis vacuus. Et tamen Mundus erit plenus, unde intelligitur quantitatem inassignabilem esse aliquid."[7] The atomism survives in 1676, although the void is abandoned, and the influence of his mathematical work is visible (this was just at the end of the period in Paris, when he was corresponding with Newton through the medium of Oldenburg). In this year occurred also his visit to London and to the Hague.

In the next period of his life, when he had for some years been occupied chiefly with mathematical matters, falls the elaboration of his argument against Descartes's theory of matter,—Descartes, who had been partly responsible for

[5] Latta, p. 300. [6] See Kabitz, p. 53. [7] Couturat, 1903, p. 10.

Leibniz's tendency toward a mechanical view. The unsatisfactory character of the views of Descartes and of Gassendi had, it is true, been pointed out by him several years before. In this later period, besides physic and pure mathematics, a third scientific interest may be noted. He refers often to Swammerdam, Leuwenhoek and Malpighi, and it is evident that he felt a genuine enthusiasm for the progress of biology, aside from the support which certain theories lent to his doctrine of preformation. But as his interest in biology is apparently subsequent to the observable beginnings of monadism, these theories were rather a confirmation than a stimulus.

To these philosophical and scientific occupations must be joined another which was no less important. This is his perfectly genuine passion for theology. Developed perhaps out of his early training, this theology, in a mind which never lost an interest it had once taken up, remained a powerful influence throughout his life. His solicitude for the orthodoxy of his philosophy was not merely policy or timidity; his theological disputations are not merely a cover for logical problems. Leibniz's theological motive is responsible for much of the psychology of his monads; it took deep root in his system, though not altogether without disturbance of the soil. The only two interpretations of Leibniz which are of any importance, that of Dillmann[8] and the superior interpretation of Russell and Couturat, minimize the significance of this motive.

"Ma métaphysique est toute mathématique, pour dire ainsi, ou la pourroit devenir," Leibniz writes to the Marquis de l'Hôpital (Dec. 27, 1694). And Russell says (p. 49) in speaking of the subject-object relation, "the whole doctrine depends, throughout, upon this purely logical tenet." Strictly speaking, this assertion is perfectly justified. For a historical account it is insufficient. Leibniz puts his prob-

[8] *Neue Darstellung der Leibnizischen Monadenlehre.*

lems into logical form, and often converts them slyly into logical problems, but his prejudices are not always prejudices of logic. The value of Leibniz's logic is to a certain extent separable from the value of his philosophy. The view of the nature of substance with which he starts is due to a logical problem. But there is no logical descent from pluralism to the view that the ego is substance. Leibniz's view of substance is derived from Aristotle, but his *theory* of substance is quite different: it is Aristotle's theory filtered through scholasticism and tinctured by atomism and theology.

When we father the problem of substance upon Aristotle, we must remember that it was a problem which he never succeeded in resolving, or pretended to have resolved. The chief inheritance of modern philosophy from his doctrine is the proposition that "substance is that which is not predicated of a subject, but of which all else is predicated" (1029*a*). Aristotle recognizes that there are various senses in which we may use the term, and various substances beside the sensible substances, which have matter. In one sense the composite of form and matter (e. g., animals and plants) is substance, in another sense substance is "the form by which the matter is some definite thing" (1041*b*). And again the substratum (1029*a*) is that of which everything is predicated. Matter certainly is not substance, because matter *qua* matter has neither limit nor the potency of limit by separation (see 1017*b*). And again the universal is more substantial than the particulars (*Metaph.*, Z 13). Wherever Aristotle pursues the concept of substance it eludes him. These tentative definitions, assumed for dialectic purposes, are abandoned in favor of that of 1041*b*. This bears, it is true, very striking resemblances to the substance of Leibniz. As to the meaning of form and the relation of formal to efficient and final

cause Aristotle remains difficult and vague, while for Leibniz the formal and efficient causes in the case of substance are identical.

There is another and very serious difficulty in the theory of Aristotle. From one of Aristotle's points of view only the individual should be real, from the other only the specific. The form is always ἀτομόν; thought analyzes and resynthesizes its constituents to give the λόγος τοῦ τὶ ἦν εἶναι. Of the subject either the whole or a part of the definition can be affirmed: thus we can define Socrates *qua* man as ζῷον δίπουν λογικόν. But predications of particular individuals belong to the attributive, not to the definitory type of judgment. In this type of judgment the predicate affirmed, although it belongs to the subject, is not a constituent of the subject's essential nature. As the essential nature of Socrates is man, anything which is not contained in the form of man in general will be attributive only and not definitory, inasmuch as it might have been otherwise. For Aristotle not all predicates are contained in their subjects. Hence there can be no definition of individuals of a species (1040*a*). The substance must be individual, in order to be the subject; it must be a "this." But the "this" cannot be composed of universals, because no number of "suches" will constitute a "this," and on the other hand it cannot be composed of other substances. We thus get two opposed views: the substance is the form of the species, in which case it breaks loose from the concrete thing and gives rise to the same difficulties which Aristotle censured in Plato; or the substance is the individual thing, in which case there is no definition and no knowledge. One view is in harmony with Aristotle's methodology, the other with his theory of elementary cognition.[9]

[9] In *An. post.* 100*a* (Chap. XIX) we are told how the knowledge of the universals arises through experience of particulars. "First principles" are arrived at by induction. What is not made clear is the status of the particulars after scientific knowledge is established.

Aristotle is here betrayed by his representation theory —the exact correspondence between constituents of propositions and constituents of things; although in other contexts he is an epistemological monist. The same incoherence appears in his account of the soul. Is the substance the compound of matter and form, or the form alone?

It was the Aristotelian problem of substance, affected by scholasticism, that Leibniz took upon his shoulders at the beginning of his career. Later in life he observes that he has been re-reading Aristotle, and that he finds much of value in him. The extent of his acquaintance with the text may be left in doubt. It is probable that he had little or no direct knowledge, that he abandoned the study of the history of philosophy almost altogether for some years, and the fresh approach to Aristotle did not produce much effect upon his subsequent work. The interest lies in Leibniz's saturation which the Aristotelian tradition—in spite of a momentary peevishness against the degenerate scholasticism in which he had been brought up—and in the compound to which the contact of this training with the speculations of contemporary science gave rise. To this particular problem the drawing of parallels and the estimating of borrowings—conscious and unconscious—is irrelevant. Nor are we here concerned with the question whether "this seemingly fantastic system can be deduced from a few simple premises."[10] The question is the actual genesis of the system. If, at the age of fifteen, Leibniz inclined to the view that substances are particular individuals and that relations exist only in the mind; if we can see that his transition to atomic materialism follows quite easily from this; if we find that his further development depended upon the way in which his scientific researches and his theological prejudices — largely an inheritance from his early training—played into each other; then we shall con-

[10] Russell, p. viii.

clude that his metaphysics and his scientific achievements —logical and mathematical—are two different values.

What is curious about Leibniz's mind is the existence of two distinct currents. As a scientist he has a clear and consistent development. Every step is justified and coherent from this point of view alone. His metaphysics is carefully built upon his scientific evolution. On the other side is a strong devotion to theology. His study of Descartes marks a stage in the development of both. Descartes's theory of matter, and Descartes's theory of self-consciousness both had their effect upon him. And it is always the same mind working, clear and cold, the mind of a doctor of the church. He is nearer to the Middle Ages, nearer to Greece, and yet nearer to us, than are men like Fichte and Hegel.

We have seen that there is a very great difference between the Aristotelian theory of substance and the nominalism deriving from it with which Leibniz starts. Both in the *Metaphysics* and in the *De anima*, it is true, Aristotle leaves the answer somewhat ambiguous. When he discusses the substance of organic beings we are apt to think that each individual is a substance—that the form of each body is an individual—one form for Socrates, and another for Callias. It is difficult to avoid this conclusion, but in general, for Aristotle as well as for Plato, whatever was merely individual was perishable and incapable of being a subject of knowledge. But if we say, with Burnet (*Greek Phil.*, p. 331) that "Plato found reality, whether intelligible or sensible, in the combination of matter and form and not in either separately," and take the same view of Aristotle, yet we cannot say that they found it in each individual as a world apart. This is an instance of the differences between Leibniz and the Greeks. In Leibniz we find the genesis of a psychological point of view; ideas tend to become particular mental facts, attributes of par-

ticular substances. If the form or principle of Aristotle were different in each man, this form would be Leibniz's soul. For the Greek the human was the typically human, individual differences were not of scientific interest; for the modern philosopher individual differences were of absorbing importance.

We may now trace the two currents which are imperfectly united in the monad. Leibniz approaches the problem of substance primarily as a physicist. "Leibniz does not begin with the problem, what is the substance of the body, what is its origin, but from this: how the principle of the body itself may be conceived" (Dillmann, p. 63). To those readers—there are still a few—who know Leibniz only through the *Monadology,* the steps to the conclusion will remain unknown. Unless we appreciate the original question we shall be unable to understand his solution of the problem of body and soul, and of the problem of our knowledge of external objects. He never asked the question, "do physical bodies exist?" but always, "what is the principle which makes physical bodies intelligible?" The answer is found in his reaction to Cartesianism. And at this point, while the problem of energy was engaging his attention, he read some of the dialogues of Plato, and was confirmed in his conclusions especially by certain parts of the "Sophist." What we get is on the one hand an explanation of the principle of matter, and on the other an idealistic metaphysic, largely influenced by Descartes, based upon self-consciousness. The latter aspect has of course been more exploited than the former.

Leibniz's account of physical matter is a much more scientific, but in some respects much cruder, explanation than Aristotle's. For Aristotle's account is fundamentally a relativistic one, i. e., "matter" has various meanings in relation to shifting points of view which form a series but are not themselves defined. There are meanings in various

contexts, but no absolute meaning; and the series of points of view, the series of contexts, has no absolute meaning either. One misses the whole point of Aristotle's theory if one regards matter as a "thing." It is—whether as primitive matter, as the four elements, or as any compounds (I mean σύνθεσεις not μίξεις) of any degree of complexity formed out of these, one side of a contrast in the mind (or imposed upon the mind) though this mind is no more absolutely definable than matter itself. (Hence Aristotle is neither an idealist, in the modern sense, nor a pragmatist.) *Materia prima* is not simply negative nor is it positive in any apprehensible way. It is simply the furthest possible extension of meaning of a concept which has arisen out of practical complexes. The next stage in the conception of matter, it will be recollected, is that of a subject possessing two out of two pairs of opposites (wet-dry, hot-cold). The *materia prima* is not *actual*, because it has no predicates; the smallest number of predicates which an actual existent can have is two. That is, whatever is merely hot, or merely dry, is not a substance but is identical with the quality itself; but whatever is hot and wet, or cold and dry, is a substance different from its predicates. These elements—the possible combinations of four qualities—are capable of transmutation into one another in a cycle which occurs in the exchange of qualities (the hot-dry becomes hot-wet, the hot-wet becomes wet-cold, etc.). The third stage of matter is that of the stable compounds of the four elements held together in various proportions. This progress is not a chemical theory in the modern sense; it is a series of points of view. The formal cause is therefore identical with the thing itself, and whether the form is there is a question of what we regard as the thing. The lump of marble is a σωρός of higher compounds of the four elements—or it is a statue. One must keep in mind the two apparently inconsistent

propositions: (1) there are no forms of individuals,[11] (2) the form and the matter compose one whole.

Aristotle is too keen a metaphysician to start from a naive view of matter or from a one-sided spiritualism. To a certain extent Leibniz keeps this middle ground too. But his metaphysics tends to fall apart, as the result of his inherited nominalism, and the fissure between his scientific and his theological interests. Starting as a physicist, Leibniz naturally assumes that matter is not a relative term but that it is (if it exists at all, of which he has no doubt) something absolute. The substantiality of matter consists then (after his defection from Cartesianism) in the concept of force. Force is not conceived as something behind matter, which could be actual without matter. But neither is it a "form" in quite the Aristotelian sense. The "real and animated point" of the *Système nouveau* is from an Aristotelian point of view merely another individual, or a form of an individual. It is purely and simply a physical explanation. It involves no theory of knowledge, because it does not take into account the point of view of an observer; it is a contrast not between matter and form, but between a particular substance and its states.

The distinction between *materia prima* and *materia secunda* (of bodies) is superficially Aristotelian. But it is really only a distinction between two ways in which matter may be considered for the purposes of the physicist. It is a distinction of uses and not of contexts. "Matter is not a relative term. The ancient distinction between matter and form does not correspond to the modern distinction, since Descartes, of matter and spirit. And the dichotomy is as strongly marked in Leibniz as in Descartes. His solution of the difficulty marks the wide gulf that separates modern from ancient philosophy. For Aris-

[11] Except of course eternal and unique individuals, like the moon, which is the only individual of its species. And for later theology, the angels.

totle matter and form were always relative, but never identical. For Leibniz matter and spirit are absolute reals, but are really (as for Spinoza) the same thing. The difference for Leibniz is that between internal and external aspects. *Materia prima* is not a stage, it is an external aspect, and even for physics he finds this aspect insufficient. He is therefore led gradually into a metaphysical conception. But from this metaphysical account of the nature of the physical universe to his doctrine of souls there is really no legitimate inference.

The theory of forces, as the substances of which material changes are the states, is not the theory of the soul which derives from his more theological interest. It is, as we have said, simply an analysis of the physical universe. Had Leibniz been quite consistent he would have gone on to explain organic and conscious activity on a strictly physical basis. This he did accomplish in some measure. His doctrine of expression (see letter to Arnauld, Oct. 6, 1687) is an account of perception consistent with a purely physical and mathematical point of view. But his transmigration[12] of human souls is muddled by the identification of soul, in the sense of personality, with the animated point; of the core of feeling of the self with the force of which it is predicated. From his physical point of view he cannot arrive at self-consciousness, so that his doctrine of force has two grounds—the theory of dynamics and the *feeling* of activity. If we refuse to consider self-consciousness a simple and single act, if making an object of oneself merely means the detachment and observation of particular states by other states, then the "force" slips out of our hands altogether. It remains "internal," it is true, in contrast with primary matter, but its internality is not a character of self-consciousness. And in this

[12] Leibniz of course explicitly repudiates any "transmigration" of monads. But when he comes to the human soul its adventures seem to be tantamount to this.

event the whole theory becomes completely naturalistic. Something is the subject, but it is not the *I* which I know, or which anybody knows. And there then remains no reason why we should longer maintain a plurality of subjects. Force becomes one. Against such a conclusion Leibniz was set, (1) because it ceases to have any value for physics, and (2) because it interferes with our claim for personal immortality. Theology and physics join forces (so to speak) to rob metaphysics of its due.

Hence two curious difficulties arise. An animated force, a monad, tends to become an animated atom. The monad exerts its activity at a point in space and time. Artefacts, as for Aristotle, are merely groups of monads without a dominant monad. Organic bodies are groups with a dominant monad. In the latter case, in the case of a human being, in what sense is my body *mine,* since it is also the bodies of other monads? The dominant monad should be the form of the body, instead of which it bears a strong resemblance to a larger or more powerful cell, and the soul would have to be located, like Descartes's, in a particular place. Russell, in contrasting Leibniz's two conflicting theories (pp. 149-150) says of the second view: "in the other theory, mind and body together make one substance, making a true unity." So they ought to do. If the mind cannot make the body into a *unum per se,* instead of a mere aggregate, the original physical theory has advanced to a point at which mind and body fall apart. The second view appears to descend from Aristotle.[13] The first appears to descend from atomism. From neither philosophy does Leibniz ever shake himself quite free.

There is, from the physical side, a sense in which the monad is truly immortal. Force is indestructible, and will continue in various manifestations. But force in this sense

[13] Leibniz actually says (letter to Arnauld, July 14, 1686): "The soul is nevertheless the form of the body."

is entirely impersonal. We cannot conceive of its persistence except by associating it with particular particles of matter. Leibniz is led by his difficulties almost to the point of either denying the existence of matter altogether, or else setting up a sort of matter which will be something real besides monads.

The second objection is connected with the generation and destruction of life. For Aristotle some account of generation and destruction is rendered possible by his provisional distinction between efficient and formal causes. Aristotle was not embarrassed by a belief in personal immortality, and his philosophy confines itself with fair success to an examination of the actual, the present life. But Leibniz's force is indestructible in a different sense from Aristotle's form.[14] It persists in time as a particular existence. The monad which is myself must have previously existed; it must have been one of the monads composing the body of father or mother (see Russell, p. 154). This theory has the disadvantages of practically denying the independence of mind from body and of separating monadhood from selfhood. It substitutes biological behavior for conscious activity.

Commencing with an analysis of the nature of matter, Leibniz is led to the view of a universe consisting of centers of force. From this point of view the human soul is merely one of these forces, and its activity should be reducible to physical laws. Under the influence of an Aristotelian doctrine of substance, he comes to conclusions which are not at all Aristotelian, by his nominalistic assumption that substances are particulars. From a materialistic atomism he is led to a spritualistic atomism. In this he shows again an important difference between the

[14] Aristotle and Plato, I am inclined to believe, owe their success in navigating between the particular and the universal, the concrete and the abstract, largely to the fact that "forms," "species," had to the Greek mind not exactly the same meaning as for us. They were concrete without being particular.

ancient and the modern world. It is illustrated in the prejudice of Aristotle against the differences between individuals of the same species which he ascribes to the perverse and unaccountable influence of matter. To the Greek, this variety of points of view would seem a positive evil; as a theory of knowledge, it would seem a refuge of scepticism; to Leibniz and the modern world, it enhances the interest of life. And yet the view of Leibniz comes, *via* nominalism, out of Aristotle himself.

From the point of view of physics we have a consistent explanation which represents a great advance upon crude materialism. But it is difficult to retain the separate forces unless we conceive of matter as a positive principle of individuation. Not that the doctrine of activity and passivity is wholly unsatisfactory.[15] Its effect is to reduce causality to function. And but for the Aristotelian influence, it might possibly have done so. Instead of monads we might then have had atomic particulars. But Leibniz sometimes confuses the mathematico-physical and the historical points of view. It is true that the future of the monad should be theoretically predictable. But Leibniz leaves the basis of prediction uncertain. Without recourse to mysticism, the reasons why a monad should pass from the unconscious to the conscious state, why a monad composing the body of father or mother should suddenly be elected to domination over a new body of monads, remain unsolved. We have seen that the notion of soul or spirit is not to be reached by the theory of monads as an explanation of the principle of matter. If it is part of Leibniz's inheritance we may inquire just what Aristotle's view of the soul was.

Leibniz's theory of soul is, like that of Descartes, derived from scholasticism. It is very remote from that of

[15] There are implicitly two views of activity and passivity. According to one, causality is a useful way of treating natural phenomena. According to the other, there is true activity in clear perception, true passivity in confused. This illustrates the mixture of motives.

either Plato, Aristotle, or Plotinus. For the Greeks, even for Plotinus, the soul is a substance in a sense which does not include personal immortality. For Aristotle there is no continuity between the stages of soul, between vegetable, animal and human life. And the definition of monads as "points of view" is, so far as I can see, entirely modern.

For Aristotle, according to his own explicit statement, there is no "soul" in general. As the species of figure to figure in general, so are the souls of various species of animal to "soul" in general (*De anima,* 414*b* 20 ff.). In the higher grades of soul the same functions persist, but in a form altered by the nature of the whole. The organs of different species are related by analogy—as root is to plant, so mouth is to animal, but mouth is not a development of root. The *De anima* is not so much a psychological as a biological treatise. We find in the animal the τροφὴ and αὔξησις of the plant, but completely altered in the addition of a new faculty—αἴσθησις. And these faculties are not sharp dividing lines, but in the ascending scale are used more and more loosely.[16] The natural species are immutable, and the difference does not consist in addition or subtraction of faculty.

There is a suggestion, but only a suggestion, of the doctrine of Aristotle in the three classes of monads. Even the lowest class of monad (*Monadology,* 19) has appetition. The second has feeling (sentiment) which is something more than αἴσθησις and includes φαντασία and perhaps διάνοια. The soul of man only has self-consciousness, a knowledge of eternal and necessary truths,—νοῦς. It seems very probable that this scheme was suggested by Aristotle[17] but there is a profound difference. The classification of Aristotle is on the basis of biological functions.

[16] Cf. 413 b 12, 432, and 414. Motion according to 413 is not a fourth species of the soul besides θρεπτικόν, αἰσθητικόν, διανοητικόν.

[17] And, in passing, it seems possible that the theory of Leibniz may have supplied a hint for the romantic evolutionism of Diderot.

These are functions of the organism as a whole, a complex substance. Plants are not ζῷα, and have no appetition. Aristotle makes much of the distinction between beings which are attached to a single place and those which move about. For Leibniz the distinction is not biological, but psychological, and is everywhere a difference of degree. The lower monads, if they had clearer perceptions, would rise in the scale. It is not a limitation of the body, but a limitation of the nature of the monad itself which establishes differences. For Leibniz the series is a continuum; for Aristotle it is not. For Leibniz desire characterizes mind; for Aristotle desire is always of the complex organism; the function of mind is solely the apprehension of the eternal and necessary truths and principles.

There is another point upon which Leibniz may have drawn his inspiration from Aristotle, and that is the "common sense." "The ideas which are said to come from more than one sense, like those of space, figure, motion, rest, are rather from common sense, that is from the mind itself, for they are ideas of the pure understanding, but they are related to the external, and the senses make us perceive them" (see Russell, p. 163). Leibniz's theory appears to be a transition between Aristotle and Kant. What Aristotle says is this: "The above (i. e., color, sound, etc.) are called *propria* of the respective senses; the perceptions common to all are motion, rest, number, figure, magnitude. These are not propria of any, but are common to all" (418*a* 17ff). Whereas Leibniz stuffs these κοινὰ into the mind, Aristotle goes no farther than to say that they are perceived κατὰ συμβεβηκὸς by all the senses. There is not, as is sometimes thought, a "common sense" which apprehends them, as the eye perceives color.[18] What is interesting in the present context is the cautious empiricism of Aristotle's

[18] Zabarella, probably the greatest of all Aristotelian commentators, is very positive on this point.

theory, contrasted with the more daring but less sound speculations of Leibniz.

The question of the relation of mind to matter is handled by Leibniz differently from either Aristotle or Spinoza. I am inclined to think that it was conceived quite independently of Spinoza. Leibniz attacks Spinoza fiercely on the ground of Spinoza's naturalism, and for his disbelief in free-will and immortality.[10] He perceives, quite correctly, that Spinoza's view of the relation of mind and body leads to a materialistic epiphenominalism. "With Spinoza the reason does not possess ideas, it is an idea." He insists that the mind and the body are not the same thing, any more than the principle of action and the principle of passion are the same thing. But he inclines to believe that the difference between mind and matter is a difference of degree, that in all created monads there is materiality. (There seems to be a relation between *materia prima* of monads and *materia prima* of matter.) Now this suggests the Aristotelian relativity of matter and form; for Aristotle the higher substances are more "formed," the percentage of crude matter seems to decrease. There is no matter and no form in an absolute sense (except the form of God, who is rather a disturbing factor). But whereas for Aristotle matter exists only in contrast with form, and formed matter may be the matter for a higher form, for Leibniz matter really exists independently of spirit, but is really spirit.

Leibniz's use of the term "entelechy" is not identical with that of Aristotle. The monad is called entelechy apparently because it is complete in itself, complete in the sense of self-sufficient; while the entelechy of Aristotle is the *completion* or actuality of something. In the *De anima* the soul is called the first entelechy of body. To be strictly consistent, Aristotle should perhaps have held that soul

[10] See Foucher de Careil: *Réfutation inédite de Spinoza par Leibniz.*

is the second entelechy, since he maintains that it is only actual when it energizes; but he is merely trying to distinguish between the form and its operation.[20] Entelechy means that the body would not be a human body without the soul. It is difficult, it is true, not to think of the soul as something added to the body (as to Galatea) or else to identify soul with the (living) body. Soul is to body as cutting is to the axe: realizing itself in its actions, and not completely real when abstracted from what it does. In the light of Aristotle's elaborate critique of earlier theories of the soul, his view is seen as an attempt to get away from the abstractions of materialism or of spiritualism with which we begin. For Aristotle reality is here and now; and the true nature of mind is found in the activity which it exercises. Attempt to analyze the mind, as a thing, and it is nothing. It is an operation. Aristotle's psychology therefore starts with psycho-physics, and ascends to speculative reason. It is only then that we perceive what mind is, and in retrospect find that it was present in the simplest sensation.

The word entelechy as used by Leibniz loses the meaning which it had for Aristotle. It becomes figurative and unimportant. Leibniz appears at first less a dichotomist than either Aristotle or Descartes. In effect, the breach between mind and matter becomes far wider than in the system of Aristotle. In order that mind may persist at all times as something distinct from the body, appeal is made to the subconscious,—a parallelism even more mystifying than that of Spinoza. With Leibniz the relation of mind and matter is closer, the relation of body and soul more remote, than with Aristotle. The weakness in Leibniz's theory of body and soul may be due to two causes. On the one hand his theological bias made separation of

[20] See *De anima*, 412*a*, 27, where δυνάμει ζωὴν ἔχοντος means having "the potentiality of functioning," not "the potentiality of soul." The above distinction between form and operation was pointed out by Zabarella.

body and soul essential; and on the other hand it was necessary, from his more strictly philosophical substances, the monads should persist after the compound substances, the bodies, which are their points of view. It is required both by his theory of substance, and by his demand for a mathematical metaphysic. The causal series which is the monad should apparently have no last term.[21] Perception (in Leibniz's general statement of expression) requires that every series should be similar both to every other series and to the series of series.[22] The same theory which demands unconscious perception seems to demand also a series which shall not terminate in time. Supposing that the destruction of individual monads shall leave the total, as an infinite number, undiminished, nevertheless the monad as a substance will have to shut up shop, and we shall be left with a number of relations relating nothing. Some sort of persistence is necessary for the system, though not the personal immortality which Leibniz is interested in supporting. It is evident that with the possibility of changes of "point of view" the meaning of prediction becomes hopelessly attenuated. Every moment will see a new universe. At every moment there will be a new series of series; but continuity makes necessary a point of view from which there shall be a permanent series of series of series.

Leibniz's theory of mind and matter, of body and soul, is in some ways the subtlest that has ever been devised. Matter is an arrested moment of mind, "mind without memory."[23] By state is not meant feeling, but the monad at any instant of time.[24] In many ways it is superior to that of Aristotle. When he turns to preformation, to the

[21] See Russell: "Recent work on the philosophy of Leibniz," *Mind,* Vol. XII, N. .S., No. 46, pp. 25-26.

[22] See Russell, *ibid.,* p. 25.

[23] *Theoria motus abstracti,* 1671; quoted in Latta, p. 230. Compare the Bergsonian theory of matter as consciousness "running down."

[24] Cf. "only indivisible monads and their states are absolutely real."

vinculum substantiale, to the immortality of the soul, we feel a certain repulsion; for with all the curious fables of the "Timaeus" or the "Physics" and Aristotle's history of animals, we know that Aristotle and Plato were somehow more secure, better balanced, and less superstitious than the man who was in power of intellect their equal.

There are two other points in monadism which direct attention to the Greeks. These are the theory of innate ideas and the theory of substance as force expressed in the "Sophist." So far as the question of indebtedness goes I think that the answer is clear enough. The views which Leibniz held were forced upon him by his own premises. He undoubtedly read Plato at a time when his own theory was not yet crystallized, but he cannot be said to have borrowed. He may be given full credit for having restored to life in a new form the doctrines of Plato and Aristotle. The monad is a reincarnation of the form which is the formal cause of Aristotle. But it is also more and less. The outstanding difference is that he sets out from an investigation of *physical* force, and his monads tend to become atomic centers of force, particular existences. Hence a tendency to psychologism, to maintain that ideas always find their home in particular minds, that they have a psychological as well as a logical existence. Leibniz on this side opened the way for modern idealism. To his anticipations of modern logic of a school opposed to absolute idealism it is unnecessary for me to point. No philosophy contains more various possibilities of development, no philosophy unites more various influences. That he did not always unite them successfully—that he never quite reconciled modern physics, medieval theology, and Greek substance, is not to be reproved when we consider the magnitude of his task and the magnitude of his accomplishment.

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LEIBNIZ'S "IMAGE OF CREATION."

IN the achievements of great men the trivial and curious frequently loom higher than the solid and substantial. At one time Kepler's fame centered largely around the pseudo-discovery of fanciful relations between the regular solids and planetary distances. Placing the icosahedron, dodecahedron, octahedron, tetrahedron and cube, one within the other at such distances that each solid was inscribed in the same sphere about which the next outer solid was circumscribed, he found that the radii of the spheres were roughly in the ratio of successive planetary distances. Only in an uncritical age could such very crude numerical resemblances command any attention, especially as there seemed to exist no causal relation between said radii and the distances of planets from the sun.

Nowadays we smile at Kepler's early speculation. Nevertheless it is a fair question to ask why the regular solids should be less likely to play a part in the mathematical theory of planetary motion than does that conic section which was destined later in Kepler's career to lend itself to the establishment of his permanent world-fame as an astronomer—why the ellipse rather than the "Platonic figures"? In Kepler's time the law of gravitation could not be appealed to for arbitration; it was still hidden away in the realm of the unknown. No *a priori* decision was within reach at that time. The answer could come only after painstaking measurements, combined with mathe-

matical deduction, which, in this case, confirmed one guess and exploded the other.

The famous mathematician Sylvester derived great satisfaction from lecturing in Baltimore on versification and displaying his skill in the making of rhymes. Rumor has it that he was fonder of his grotesque booklet, the *Laws of Verse*, than of any of his great mathematical discoveries.

Thus it was also with Leibniz. He was strangely partial to a discovery of very minor importance that he made relating to the so-called binary numbers, which are constructed on the scale of 2 instead of 10 and require only two symbols, namely 0 and 1. In his scale, 1 is written 1, 2 is written 10, 3 is written 11, 4 is written 100, 5 is written 101, and so on.

The charm of Kepler's regular-solid-theory of planetary distances lay in the unexpected relation thought to exist between magnitudes so foreign to each other that not the remotest cause for such intimate relation could be imagined. Sylvester's fantastic performances lay in the acrobatic groupings of words similar in their terminal sounds. The fascination in the dual arithmetic of Leibniz lay in the philosophical and religious mysticism associated with it. The 0 and 1, by which any number could be represented in that system, symbolized the creation of everything out of nothing; it afforded a phase of religious mystery which was thought to be helpful in the conversion of the heathen. The idea of Leibniz was based upon sound psychology. The mind of man delights in figures of speech, in analogies, in images. Here was an "*imago creationis,*" truly novel and simple. The fact that it rests upon a mathematical basis was no drawback. Had not number-theory figured prominently in ancient religious mysticism?

With Leibniz his dual arithmetic was more than a passing fancy. He had reflected on this subject for over

twenty years, before he permitted an account of his meditations to appear in type. He made his first full statement of the binary scale and its symbolic interpretation in a letter written on January 2, 1697, to Duke Rudolph August of Brunswick. A little later, on May 17, 1698, Leibniz touched upon this subject in a letter to Johann Christian Schulenburg of Bremen, in which he states that his first thoughts on this matter antedate the year 1678. Accordingly his first ideas on binary numbers go back to the time when he was making his marvelous invention of the differential calculus. In April, 1701, Leibniz wrote enthusiastically on these numbers to John Bernoulli, then at Groningen in the Netherlands. Two years later, on July 12, 1703, he sent an account of his new arithmetic to Fontenelle, the secretary of the French Academy of Sciences, and it was published in 1703 under the title "Explication de l'arithmétique binaire," in the *Mémoires de l'Académie des Sciences de Paris*. This was the earliest appearance of this subject in print. The perusal of this article convinces the reader that Leibniz regarded it with parental pride.

A letter of Leibniz, written some years before, contains a statement which, we believe, has reference to the binary scale. It is a letter of September 8, 1690, sent to Placcius, who was professor of philosophy in the gymnasium in Hamburg. We may state parenthetically that this letter is of general interest, aside from its probable allusion to the binary scale. It reveals his ideas on the most profitable course of mathematical study and discloses information regarding the Hamburg mathematical club which ranks as the earliest organization of that sort known in mathematical history. We translate as follows:

"Recently I saw a book which deals in the German language with numerical problems, from which I gather that in Hamburg a few prominent arithmetical experts have

combined and formed a society with which others in that vicinity have become affiliated, and that Meissner, one of your countrymen and a teacher of arithmetic, is the leader of this movement. I am much pleased with this organization and I expect from it excellent things if they can make up their minds to expend their efforts upon matters which will enlarge the boundary of science; for to spend the time on special problems is not quite worthy of this undertaking, unless these problems are of particular elegance and usefulness or help to enlarge the field of the general method itself. Nothing is simpler than to collect problems which are easy for us who know the mode of procedure, but which cause others unnecessary labor. One should endeavor to perfect analysis itself, and I do not believe that there is any one in Germany who has acted in this matter with more zeal—not to say with greater success—than have I myself....

"I am also in possession of an invention for the construction of algebraic tables which, if once made available, would simplify computation and would afford to analysis almost as much aid as do the sine tables and logarithmic tables in ordinary arithmetic."

Does the last paragraph in this quotation refer to the binary system? In the letter written some years later (Jan. 2, 1697) to Duke Rudolph August, he says of this system:

"At the bottom of this there are so many wonderful things to see, useful also in the advancement of science, that some members of the Hamburg Arithmetical Society, whose diligence and aims are praiseworthy, could enjoyably direct their thoughts upon this and, as I can assure them, find things therein which would bring no little renown to them, and also to the German nation for having been first brought forth in Germany. For I see that from this mode of writ-

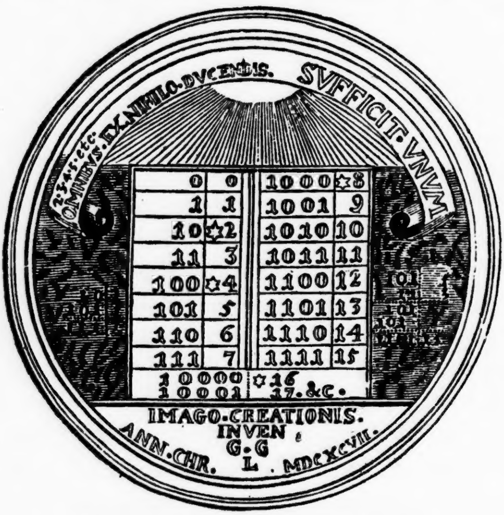
ing numbers there can be derived wonderful advantages profitably applicable also in ordinary arithmetic."

And what are the advantages which can be claimed for the binary system? In the first place it has no multiplication table beyond $1 \times 1 = 1$. Practically all operations can be performed by mere addition and subtraction. Consider for example the multiplication of 2 by 3. In the binary system $10 \times 11 = 110$, $11 \times 11 = (10 + 1)11 = 110 + 11 = 1001$. To be sure nearly four times as many figures must be written down in the binary scale as in the decimal scale, but the absence of a multiplication table is a vital gain. "Calculation as an effort of mathematical thought," says a recent writer, "might be said to be entirely dispensed with, and the labor of the brain to be all transferred to the eye and hand."

In his letter of Jan. 2, 1697, Leibniz accompanies his New Year's greetings to Duke Rudolph August by the remark: "That I shall not come this time altogether empty, I send you a symbol of what I recently had the honor to mention to you. It is in the form of a thought-penny or medal; and while my design is trifling and to be improved according to one's taste, yet the thing itself is of such a nature that it would seem worthy to be exhibited to posterity in silver, if such were to be stamped by the command of your gracious Highness. For one of the chief tenets of Christian faith, one of those which have met with the least acceptance on the part of the worldly wise and are not easily imparted to the heathen, relates to the creation of all things out of nothing by the all-power of God. It can be rightly claimed that nothing in the world better represents this, indeed almost proves it, than the origin of number in the manner represented here, where, by the use simply of unity and zero or nothing, all numbers originate. In nature and philosophy it will hardly be possible to find a better symbol of this mystery, for which reason

there is placed upon the design of the medal, *Imago Creationis.*"

According to Leibniz, this image shows that God created all things well: "For while in the ordinary mode of writing numbers there can be recognized no order or definite sequence of characters or relations, there appears now, since one can see the innermost recesses and the primitive states, a wonderfully beautiful order and harmony which cannot be improved upon, and is exhibited, first of all, in a



LEIBNIZ'S IMAGO CREATIONIS.

fixed rule of alternation by which we can write down all members without computation and without aid of memory as far as we please, if we put in the first column on the right, or in the last position, alternately underneath each other: 0, 1, 0, 1, 0, 1, 0, 1, etc.; and put in the next column (proceeding from right to left): 0, 0, 1, 1, 0, 0, 1, 1, etc.; and in the third column: 0, 0, 0, 0, 1, 1, 1, 1, 0, 0, 0, 0, 1, 1, 1, 1, etc.; in the fourth: 0, 0, 0, 0, 0, 0, 0, 0, 1, 1, 1, 1, 1, 1, 1, 1, and so on,.... This continuous order and beauty

can be seen in the small table on the medal, as far as 16 or 17....

"To explain the other parts of the medal I have marked the principal places with an asterisk, namely 10 or 2, 100 or 4, 1000 or 8, 10000 or 16; for if one examines just these, one derives therefrom the structure of the other numbers. Why, for instance 1101 stands for 13 is as follows:

$$\begin{array}{rr} 1 & 1 \\ 00 & 0 \\ 100 & 4 \\ 1000 & 8 \\ \hline 1101 & 13 \end{array}$$

and similarly for all others. On the sides of the table on the medal I have placed an example in addition and one in multiplication, that we may understand the operations and notice that the ordinary rules of computation hold here also—even though there is no intention on our part to use these modes of computation in any other way than to discover and exhibit the mysteries of numbers....

"If, as in perspective, one examines things from the proper point of view, one can see their symmetry. And this stimulates us more and more to praise and love the wisdom, goodness and beauty of the Highest Goodness, from whom all goodness and beauty flows. Hence, as I now write to Pater Grimaldi in China, a Jesuit and the president of the mathematical tribunal there, with whom I became acquainted in Rome,....to whom I thought it well to communicate this representation of numbers, with the hope—since, as he himself stated, the monarch of this extensive empire is a lover of arithmetic who learned from Pater Verbiest, Grimaldi's predecessor, European methods of computation—that this image of the mystery of creation might serve to bring more and more before his eyes the excellencies of the Christian faith."

To render this medal, designed as an image of creation, still more attractive and artistic, Leibniz suggested that it should also represent light and darkness, the spirit of God moving upon the face of the waters. As a motto he chose the following:

> "2, 3, 4, 5, etc. 0. *Omnibus ex nihilo ducendis sufficit unum.*" (To make all things from nothing, unity suffices.)

The binary arithmetic of Leibniz captured the attention of many mathematical writers. The mystic element put it in the class of mathematical recreations. Even Laplace, the heterodox, in his famous *Essai philosophique sur les probabilités,* speaks of it and its use in Chinese missions.

A curious blunder in mathematical history grew out of the binary arithmetic of Leibniz. The French Jesuit Bouvet, a missionary at Pekin and a zealous student of Chinese antiquities, learned of Leibniz's binary arithmetic and its theological interpretation. By the exercise of ingenious powers of coordination he found therein a key to the explanation of the *Cova,* or lineations of Fohi, the founder of the empire. They consisted of eight sets of three lines, either entire or broken lines, arranged in a circle. These *Cova* were held in great veneration in China, being suspended in all temples and, though not understood, were supposed to conceal great mysteries, embracing all true philosophy, both human and divine. Now Bouvet thought he had penetrated to the very depths of these mysteries when he announced triumphantly the discovery that in the *Cova* figures, the short lines meant 0 and the long lines meant 1, that Fohi possessed the principles of the binary arithmetic and that the *Cova* bore testimony to the unity of the Deity. Bouvet explained all this in a letter to Leibniz, dated Nov. 14, 1701. Leibniz, in turn, reported these findings in the paper to the Paris Academy which, as already related, was published in its *Mémoires* of 1703.

This application of the binary arithmetic to the interpretation of ancient oriental symbols afforded Leibniz profound pleasure. To the mathematician it meant the discovery that the Chinese had been in very early times in possession of binary arithmetic with its great principle of local value and the use of the zero. For the next 250 years the Chinese origin of this principle and of the zero appeared to be an established fact in mathematical history and was accepted as true even by the great mathematical historian of the nineteenth century, Moritz Cantor of Heidelberg, in his earlier publications. However, in 1863 Cantor became convinced that the traditional interpretation was incorrect, that the *Covas* of Fohi are not numbers at all, but have a physical significance, representing, respectively, air, rain, water, mountain, earth, thunder, fire, wind.

Thus it is seen that Leibniz's very minor invention of dual arithmetic was to him an object of contemplation for over a quarter of a century; it afforded him a satisfaction out of all proportion to its importance. He corresponded on the subject with mathematicians and religious teachers. It gave rise to an interesting chapter in modern religious mysticism and in the annals of foreign missions; it led to a blunder in the history of numeral notations which persisted for two centuries and a half, until the time of a great mathematical historian who is still living. It was the point of departure of interesting speculations as to the relative advantages of numeral notations whose bases are powers of 2, that is, the bases 2, 4 and 8.

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LEIBNIZ'S MONADS AND BRADLEY'S FINITE CENTERS.

NO philosopher is more fantastic than Leibniz in presentation, few have been less intelligently interpreted. At first sight, none is less satisfactory. Yet Leibniz remains to the end disquieting and dangerous. He represents no one tradition, no one civilization; he is allied to no social or literary tendency; his thought cannot be summed up or placed. Spinoża represents a definite emotional attitude; suggestive as he is, his value can be rated. Descartes is a classic, and is dead. "Candide" is a classic: Voltaire was a wise man, and not dangerous. Rousseau is not a classic, nor was he a wise man; he has proved an eternal source of mischief and inspiration. Reviewing the strange opinions, almost childish in *naiveté,* of birth and death, of body and soul, of the relation between vegetable and animal, of activity and passivity—together with the pitiful efforts at orthodoxy and the cautious ethics of this German diplomat, together with his extraordinary facility of scientific insight, one is disconcerted at the end. His orthodoxy is more alarming than others' revolution, his fantastic guesses more enduring than others' rationality.

Beside the work of Russell and of Couturat I have found only one author of assistance in attempting to appreciate the thought of Leibniz. In Bradley's *Appearance and Reality* I seemed to find features strikingly similar to those of monadism. So that re-reading Leibniz I cannot

help thinking that he was the first to express, perhaps half unconsciously, one of those fundamental varieties of view which perpetually recur as novelties. With his motives, logical and otherwise, I am not here concerned. I only wish to point out, and leave for consideration, certain analogies.

That monadism begins with Leibniz I think will be conceded. It is characteristic of the man that everything about his monads, except the one essential point which makes them his own, he may have borrowed from an author with whom he was certainly acquainted. Bruno's theory has everything in common with that of Leibniz except this one point. A kind of pre-established harmony, the continuity of animal and vegetable and of organic and inorganic, the representation of the whole in the part, even the words *monadum monas*: these points of identity one finds.[1] But the monad of Bruno has this difference: it has windows. And it is just the impenetrability of the Leibnizian monads which constitutes their originality and which seems to justify our finding a likeness between Leibniz and Bradley. In any case, there is no philosopher with whom the problem of sources is less important than with Leibniz. The fact that he could receive stimulation from such various sources and remain so independent of the thought of his own time[2] indicates both the robustness and the sensitiveness of genius. He has studied Thomas, and probably with great care the *Metaphysics* and the

[1] See H. Brunnhofer, *G. Bruno's Lehre vom Kleinsten als die Quelle der praestabilierten Harmonie von Leibniz* (Leipsic, 1890), for quotations, e. g.: *De trip. min.*: "Deus est monadum monas." Also *Spaccio della bestia trionfante*: "In ogni uomo, in ciascuno individuo si contempla un mondo, un universo." Brunnhofer even traces the window metaphor back to the Song of Solomon: "Prospiciens per fenestras."

[2] At least he affirms his independence. In 1679 he writes to Malebranche that as when he began to meditate he was not imbued with Cartesian opinions, he was led to "entrer dans les choses par une autre porte et decouvrir de nouveaux pays." He is also inclined to speak rather slightingly of Spinoza. See Wendt, *Die Entwickelung der Leibnizischen Monadenlehre bis zum Jahre 1695* (Berlin, 1886). The germs of monadism appear as early as 1663.

De anima, but he is not an Aristotelian; he was probably profoundly struck by the passage *Sophistes* 247e, but any one who has read his panegyric of the Phaedo (*Discourse,* XXVI) will probably agree that his praise is more the approval of posterity than the interpretation of discipleship. Leibniz's originality is in direct, not inverse ratio to his erudition.

More than multiplicity of influences, perhaps the multiplicity of motives and the very occasional reasons for some of Leibniz's writings, make him a bewildering and sometimes ludicrous writer. The complication of his interests in physics, his interests in logic, and his equally genuine interest in theology, make his views a jungle of apparent contradictions and irrelevancies. His theory of physical energy, for example, leads to an unsound metaphysical theory of activity, and his solicitude for the preservation of human immortality leads to a view which is only an excrescence upon monadism,[3] and which is in every way less valuable than Aristotle's. Thus there are features of the theory which are inessential. When we confine our attention to the resemblances between Leibniz's and Bradley's views, we will find I think that they cover everything essential. These are (1) complete isolation of monads from each other; (2) sceptical theory of knowledge, relativistic theory of space, time, and relations, a form of anti-intellectualism in both writers; from which follows (3) the indestructibility of the monads; (4) the important doctrine of "expression."[4] Certain distinctions of Bradley's, as the (relative) distinction between finite centers and selves, are also implicit in Leibniz. The relation of soul and body, the possibility of pan-psychism, the knowledge of soul by soul, are problems which come to closely similar solutions in the two philosophies.

[3] It leads Leibniz almost to the admission that persistence in the case of the lower types of monad is meaningless. Cf. *Discourse,* XXXIV.

[4] See Letter to Arnauld, Oct. 6, 1687.

I suggest that from the "pluralism" of Leibniz there is only a step to the "absolute zero" of Bradley, and that Bradley's Absolute dissolves at a touch into its constituents.

In the first place, Leibniz's theory of degrees of perfection among monads approximates to a theory of degrees of reality. Mr. Russell has pointed out how easy a step it would have been for Leibniz to have made reality the subject of all predicates. The world consists of simple substances and their states. The subject is never, even from a timeless point of view, merely equivalent to the sum of its states; it is incapable of exhaustion by any addition of predicates. The question with which Leibniz attempted to cope in his first thesis, and the question which he was never able satisfactorily to settle, was what makes a real subject, what the principle of individuation is. Nowhere in the correspondence with Arnauld do we find a trustworthy mark of differentiation between substantial and accidental unities. If everything which can have predicates, everything which can be an object of attention is a substance, the whole theory falls to the ground; but if this is not the case, we shall either be obliged to make reality the subject of all predicates, or we shall be forced to distinguish, as do some idealists, between judgments and pseudo-judgments, and the logical basis for monadism fails. If we cannot find by inspection an obvious and indubitable token of difference between the substantial and the accidental, we shall in the end find substantiality only in reality itself; or, what comes to the same thing, we shall find degrees of substantiality everywhere. In the latter case substance becomes relative to finite and changing points of view, and in the end again we must seek refuge in the one substance, or resign ourselves to find no refuge at all.

This omnipresence of substance, in degree, comes very near at times to being Leibniz's true doctrine. "One thing

expresses another, in my use of the term," he says, "when there is a constant and regulated relation between what can be said of the one and of the other...Expression is common to all forms, and is a class of which ordinary perception, animal feeling, and intellectual knowledge are species...Now, such expression is found everywhere, because all substances sympathize with one another and receive some proportional change corresponding to the slightest motion in the whole universe"; and further in the same letter "you object that I admit substantial forms only in the case of animated bodies—a position which I do not, however, remember to have taken."[5] We remark also that the lowest monads are in no very significant sense persistent: "The result from a moral or practical standpoint is the same as if we said that they perished in each case, and we can indeed say it from the physical standpoint in the same way that we say bodies perish in their dissolution."[6] The permanence of these monads seems to assert itself in order to save a theory.

There is indeed a point of view, necessary even in the severest monism, from which everything, so far as it is an object, so far as it can be assigned predicates, is equally real. But if we recognize the relativity of the point of view for which reality is merely the fact of being an object from that point of view, then the only criterion of reality will be completeness and cohesion. Suppose that some of the objects from a point of view are not direct objects (things), but other points of view, then there is no phenomenal test of their reality, *qua* points of view. So far as we cannot treat them as things, the only objective criterion of the reality will be their perfection. In any system in which degrees of reality play a part, reality may be defined in terms of value, and value in terms of reality.

Leibniz does not succeed in establishing the reality of

[5] To Arnauld, Oct. 6, 1687. [6] *Discourse,* XXXIV.

several substances. On the other hand, just as Leibniz's pluralism is ultimately based upon faith, so Bradley's universe, actual only in finite centers, is only by an act of faith unified. Upon inspection, it falls away into the isolated finite experiences out of which it is put together. Like monads they aim at being one; each expanded to completion, to the full reality latent within it, would be identical with the whole universe. But in so doing it would lose the actuality, the here and now, which is essential to the small reality which it actually achieves. The Absolute responds only to an imaginary demand of thought, and satisfies only an imaginary demand of feeling. Pretending to be something which makes finite centers cohere, it turns out to be merely the assertion that they do. And this assertion is only true so far as we here and now find it to be so.

It is as difficult for Bradley as for Leibniz to maintain that there is any world at all, to find any objects for these mirrors to mirror. The world of both is ideal construction. The distinction between "ideal" and "real" is present to Leibniz as well as to Bradley. The former's theory of space is, like the latter's, relativistic, even qualitative.[7] Relations are the work of the mind.[8] Time exists only from finite points of view. Nothing is real, except experience present in finite centers. The world, for Bradley, is simply the *intending* of a world by several souls or centers. "The world is such that we can make the same intellectual construction. We can, more or less, set up a scheme in which every one has a place, a system constant and orderly, and in which the relations apprehended by each percipient coincide...Our inner worlds, I may be told, are divided from each other, but the outer world of

[7] See *Appearance*, p. 37; Letter to Arnauld, April 30, 1687.

[8] "As regards space and time, Leibniz always endeavored to reduce them to attributes of the substances in them. Leibniz is forced to the Kantian view that relations, though veritable, are the work of the mind." Russell, p. 14.

experience is common to all; and it is by standing on this basis that we are able to communicate. Such a statement would be incorrect. My external sensations are no less private to myself than are my thoughts or my feelings. In either case my experience falls within my own circle, a circle closed on the outside; and with all the elements alike, every sphere is opaque to the others which surround it. With regard to communicability, there is in fact not any difference of kind, but only of degree...It is not true that our physical experiences have unity in any sense which is inapplicable to the worlds we call internal...In brief, regarded as existence which appears in a soul, the whole world for each is peculiar and private to that soul... No experience can lie open to inspection from outside; no direct guarantee of identity is possible...That real identity of ideal content, by which all souls live and move, cannot work in common save by the paths of external appearance."[9]

Perhaps this is only a statement of a usual idealistic position, but never has it been put in a form so extreme. A writer to whose words Mr. Bradley would probably subscribe, Professor Bosanquet, formulates the orthodox view: "No phase in a particular consciousness is merely a phase in that consciousness, but it is always and essentially a member of a further whole of experience, which passes through and unites the states of many consciousnesses.[10] This view Mr. Bradley also holds. But he more often emphasizes the other aspect. Each finite center is, "while it lasts," the whole world. The world of practice, the world of objects, is constructed out of the ideal identities intended by various souls.

For Bradley, I take it, an object is a common intention of several souls, cut out (as in a sense are the souls them-

[9] *Appearance*, p. 343ff.
[10] *Principle of Individuality and Value*, p. 315.

selves) from immediate experience. The genesis of the common world can only be described by admitted fictions, since in the end there is no question of its origin in time: on the one hand our experiences are similar because they are of the same objects, and on the other hand the objects are only "intellectual constructions" out of various and quite independent experiences. So, on the one hand, my experience is in principle essentially public. My emotions may be better understood by others than by myself; as my oculist knows my eyes. And on the other hand everything, the whole world, is private to myself. Internal and external are thus not adjectives applied to different contents within the same world; they are different points of view.

I will pass now to another consideration. Is the finite center or the soul the counterpart to the monad? It is very difficult to keep the meanings of "soul," "finite center," and "self" quite distinct. All are more or less provisional and relative. A self is an ideal and largely a practical construction, one's own self as much as that of others. My self "remains intimately one thing with that finite center within which my universe appears. Other selves on the contrary are for me ideal objects."[11] The self is a construction in space and time. It is an object among others, a self among others, and could not exist save in a common world. The soul (as in the passage quoted at length) is almost the same as finite center. The soul, considered as finite center,[12] cannot be acted upon by other entities, since a finite center is a universe in itself." "If you confine your attention to the soul as a soul, then every possible experience is more than what happens in and to this soul. You have to do with psychical events which qualify the soul, and in the end these events, so far as you are true to your idea, are merely states of the soul. Such

[11] *Truth and Reality*, p. 418.

[12] "A soul is a finite center viewed as an object existing in time with a before and after of itself," *ibid.*, p. 414.

a conception is for certain purposes legitimate and necessary..."[13] Change, accordingly, cannot be due to any agency outside of these states themselves; it can only be, "in every state of a substance, some element or quality in virtue of which that state is not permanent, but tends to pass into the next state. This element is what Leibniz means by activity."[14]

The soul only differs from the finite center in being considered as something not identical with its states. The finite center so far as I can pretend to understand it *is* immediate experience. It is not in time, though we are more or less forced to think of it under temporal conditions. "It comes to itself as all the world and not as one among others. And it has properly no duration through which it lasts. It can contain a lapse and a before and after, but these are subordinate."[15] The finite center in a sense contains its own past and future. "It has, or it contains, a character, and on that character its own past and future depend."[16] This is more clearly the case with the soul. But it would be untrue to go on and declare that the soul "bears traces" of everything that happens to it. It would be a mistake to go on, holding this view of the soul, and distinguish between various grades of soul according to faculty. This would be to confuse the soul which is a whole world, to which nothing comes except as its own attribute and adjective, with the soul which can be described by its way of acting upon an environment. In this way Leibniz thrusts himself into a nest of difficulties. The concepts of center, of soul, and of self and personality must be kept distinct. The point of view from which each soul is a world in itself must not be confused with the point of view from which each soul is only the function of a physical organism, a unity perhaps only par-

[13] *Ibid.*, p .415.
[14] Russell, p. 44.
[15] *Truth and Reality*, p. 410.
[16] *Ibid.*, p. 411.

tial, capable of alteration, development, having a history and a structure, a beginning and apparently an end. And yet these two souls are the same. And if the two points of view are irreconcilable, yet on the other hand neither would exist without the other, and they melt into each other by a process which we cannot grasp. If we insist upon thinking of the soul as something *wholly* isolated, as *merely* a substance with states, then it is hopeless to attempt to arrive at the conception of other souls. For if there are other souls, we must think of our own soul as more intimately attached to its own body than to the rest of its environment; we detach and idealize some of its states. We thus pass to the point of view from which the soul is the entelechy of its body. It is this transition from one point of view to another which is known to Mr. Bradley's readers as transcendence. It is the failure to deal adequately with transcendence, or even to recognize the true nature of the problem, which makes Leibniz appear so fantastic, and puts him sometimes to such awkward shifts.

Thus Leibniz, while he makes the soul the entelechy of the body, is forced to have recourse to the theory of the dominant monad. Now I contend that if one recognizes two points of view, which are irreconcilable and yet melt into each other, this theory is quite superfluous. It is really an attempt to preserve the reality of the external world at the same time that it is denied, which is perhaps the attempt of all pan-psychism: to substitute for two concepts which have at least a relative validity in practice—consciousness and matter—one which is less useful and consequently less significant, animated matter. So far as my body is merely an adjective of my soul I suppose that it needs no outside explanation; and so far as it possesses an independent reality it is quite unnecessary to say that this is because it is compounded of elements

which are adjectives of other souls or monads. Leibniz has here done no more than to add to the concepts of psychical and physical a third and otiose concept.

The monad in fact combines, or attempts to combine, several points of view in one. Because Leibniz tries to run these different aspects together, and at the same time refuses to recognize that the independence and isolation of the monads is only a relative and partial aspect, he lets himself in for the most unnecessary of his mysteries—the pre-established harmony. Bradley turns the Absolute to account for the same purpose. "The one Absolute" knows itself and realizes itself in and through finite centers. "For rejecting a higher experience," Mr. Bradley says, "in which appearances are transformed, I can find no reason."[17] But what we do know is that we are able to pass from one point of view to another, that we are compelled to do so, and that the different aspects more or less hang together. For rejecting a higher experience there may be no reason. But that this higher experience explains the lower is at least open to doubt.

Mr. Bradley's monadism is in some ways a great advance beyond Leibniz's. Its technical excellence is impeccable. It unquestionably presents clearness where in Leibniz we find confusion. I am not sure that the ultimate puzzle is any more frankly faced, or that divine intervention plays any smaller part. Mr. Bradley is a much more skilful, a much more finished philosopher than Leibniz. He has the melancholy grace, the languid mastery, of the late product. He has expounded one type of philosophy with such consummate ability that it will probably not survive him. In Leibniz there are possibilities. He has the permanence of the pre-Socratics, of all imperfect things.

London, England. T. Stearns Eliot.

[17] *Truth and Reality*, p. 413.

THE MANUSCRIPTS OF LEIBNIZ ON HIS DISCOVERY OF THE DIFFERENTIAL CALCULUS.

APART from the intrinsic interest which the autograph writings, and more particularly the earlier efforts, of any of the prime movers in any branch of learning possess for the historical student, there is a special interest attached to the manuscripts and correspondence of Leibniz. They are invaluable as an aid to the study of the part that their author played in the invention and development of the infinitesimal calculus. More especially is this so in the case of Leibniz; for the matter, upon which this essay is founded, unearthed by Dr. C. I. Gerhardt in a mass of papers belonging to Leibniz that had been preserved in the Royal Library of Hanover, contained holographs previously unpublished.

The most important of these, for our purpose, were edited, with full notes and a commentary, by Gerhardt, in three separate volumes, under the respective titles:[1]

1. *Historia et Origo Calculi Differentialis, a G. G. Leibnizio conscripta.* Hanover, 1846.
2. *Die Entdeckung der Differentialrechnung durch Leibniz.* Halle, 1848.
3. *Die Geschichte der höheren Analysis; erste Abtheilung, Die Entdeckung der höheren Analysis.* Halle, 1855.

[1] Because of the length and mathematical character of many of the footnotes to the Leibniz translations it has been found necessary to have them follow consecutively after the text. See "Notes," page 611.

The present time, the two hundredth anniversary of the death of Leibniz, would seem to be a most suitable one for publishing an English translation of these manuscripts.

For the present purpose, it will be convenient to group the manuscripts in two sections, of which the first will consist of Leibniz's own account of his work. Under the heading § I below is given a fairly literal translation of a postscript from Leibniz to Jakob (i. e., James) Bernoulli, "which was written from Berlin in April 1703, and then cancelled and a postscript on a totally different subject substituted."[2] This is a communication to a more or less intimate friend. It is therefore naturally not such a considered composition as the second account that Leibniz gives of his work in the *Historia* mentioned above, of which a full translation is given below under the heading § II. It is important to bear this point in mind when comparing the two accounts together, for any slight discrepancies that may be noticed are, feasibly at least, to be accounted for by the different circumstances of the compositions. The latter account bears the impress of being fairly fully revised and made ready for press, and the facts marshalled to make an impressive or, as some would have it, plausible whole; it was probably finished just before the death of Leibniz, and represents his answer to the *Commercium Epistolicum* of unsavory memory. The death of Leibniz in November 1716 was probably the cause which prevented its publication, or at least the chief reason.

It is not my intention to enter into a discussion about the *Commercium Epistolicum*; this has probably had the last word said upon it that it is possible to say with the help of the existing authentic material that is possessed by the present-day historians of mathematics. Further, I hold quite other views as to the possible source of Leibniz's inspiration, if indeed he is not to be credited with

perfectly independent discovery. I will therefore, as far as I may, refrain from allusion to the *Commercium Epistolicum,* except to second the plea of its perfectly disgraceful unfairness, as made by Leibniz.[3] I have suggested above that the *Historia* was intended by Leibniz as a statement of his side of the case, and as an answer to the attack made upon him. This account of his work, although written in the third person, "by a friend who knew all about the matter,"[4] is, on the authority of Gerhardt, undoubtedly by Leibniz himself. Without in any way impugning this authority, I cannot help thinking it would have been more satisfactory if I could have included herein photographic copies of parts of this manuscript; but this is impossible at the time of writing.

The reasons for the delay in the preparation of the *Historia* are stated in the manuscript itself; and later I shall have occasion to discuss these. In order that the remarks made may in all cases be perfectly intelligible, I must here give a very short account[5] of the history of the quarrel up to the time of the publication of the *Commercium Epistolicum* in 1712.

The matter was first started in the year 1699 by Fatio de Duillier, a Swiss mathematician who had been living in London since 1691; he was a correspondent of Huygens, and from letters that Fatio sent to Huygens[6] it would appear that the attack had been quietly in preparation for some time. Whether he had Newton's sanction or not cannot be ascertained, yet it seems certain from the correspondence that Newton had given Fatio information with regard to his writings. Fatio then concludes that Newton is the first discoverer and that Leibniz, as second discoverer, has borrowed from Newton. These accusations hurt Leibniz all the more, because he had deposited copies of his correspondence with Newton in the hands of Wallis for publication. As Fatio was a member of the

Royal Society, Leibniz took it for granted that Fatio's attack was with the approval of that body; he asked therefore that the papers in the hands of Wallis should be published in justice to himself. He received a reply from Sloane, one of the secretaries of the Society, informing him that his assumption with regard to any such participation of the Society in the attack was groundless; and in consequence of this he took no further notice of the matter, and the whole thing lapsed into oblivion.

In the year 1708 the attack against Leibniz was renewed by Keill; and the charge that Leibniz had borrowed from Newton was most directly made. Leibniz had nobody in England who was in a position to substantiate his claims, for Wallis had died in 1703; so he appealed directly to the Royal Society. This body in consequence appointed a commission composed of members of the Society to consider the papers concerned in the matter. Their report appeared in the year 1712 under the title of *Commercium Epistolicum D, Johannis Collins et Aliorum de Analysi promota, jussu Societatis Regiae in lucem editum.*

Leibniz did not return to Hanover, from a tour of the towns of Italy on genealogical research work, until two years later; so that the date of the *Historia* is definitely established to have between 1714 and 1716, the date of his death. The dates allow us to account for the similarity between the two reports he gives of his work, in the postscript and the *Historia,* and also for any slight discrepancies between them.

Let us first, however, try to find a reason why the postscript was written, and having been written why it was cancelled. In the *Acta Eruditorum* (Leipsic) for January 1691, James Bernoulli said that Leibniz had got his fundamental ideas from Barrow;[7] but in a later number, that for June 1691, he admitted that Leibniz was far in advance of Barrow, though both views were alike in some ways.[8] One

is inclined to wonder whether this admission was a result of Leibniz's reputed personality and charm; but as Leibniz seems to have been stationed at Wolfenbüttel and Bernoulli at Basel at this time a personal interview would seem improbable, and a more feasible suggestion would seem to be a reasoned remonstrance by letter from Leibniz. It is to be noticed that Bernoulli does not exactly retract his statement that Leibniz had Barrow to thank for the fundamental ideas, he only states that in spite of the similarities there are also dissimilarities in which Leibniz stands far above Barrow.[9] I am inclined to think he is simply comparing the method of Leibniz with the differential triangle method of Barrow, and that Bernoulli even has not noticed that Barrow has propositions that are the geometrical equivalents of the differentiation of a product, quotient and powers of the *dependent* variables.

It seems to me that at this time Leibniz, though he does not forget his insinuation, has to lay all thoughts of combating it aside; for Gerhardt apparently found no other letters or other manuscripts referring to the matter prior to that of 1703. At a certain time later, judging from the first paragraph of the intended postscript, he would appear to have referred to the matter again, and to have called forth from the Bernoullis an excuse or a justification of the statements in the *Acta Eruditorum,* together with some expression of their surprise that he should have been upset over them. The reason may have been that it got to the ears of Leibniz that the opinion was not confined to the Bernoullis, for Leibniz says "....you, your brother, *or any one else.*"[10]

Thus much we may guess as to the occasion that prompted the writing of the postscript; now let us try to find the reason for its being cancelled. Fatio's attack seems to have been precipitated through pique at having been left out by Leibniz in a list of mathematicians alone capable

of solving John Bernoulli's problem of the line of quickest descent.[11] "He published a memoir on the problem, in which he declared that he was obliged by the undeniable evidence of things to acknowledge Newton, not only as the first, but as by many years the first, inventor of the calculus; from whom, whether Leibniz, the second inventor, borrowed anything or not, he would rather *those who had seen Newton's letters and other manuscripts should judge than himself.*" The attack in itself is cowardly, in that Fatio does not dare to make a direct assertion, only an insinuation that is far more damaging, since it suggests that to those who have seen the papers of Newton the matter could not be in the slightest doubt. Leibniz replied by an article in the *Acta Eruditorum,* for May 1700, in which he cited Newton's letters, as also the testimony which Newton rendered to him in the *Principia,*[12] as proof of his claim to an independent authorship of his method. A reply was sent by Duillier, which the editors of the *Acta Eruditorum* refused to publish. This would probably be in 1701; and I suggest that Leibniz had probably now come to the conclusion that it would be wiser to let the matter of Barrow drop and attend to the affair with Newton. When he, unwisely, started the controversy once more by a review (containing what was taken to be an implied sneering allusion to Newton) of the *Tractatus de Quadratura Curvarum,* published by the latter with his Optics in 1708,[13] and thus drew upon himself the attack by Keill, he gladly allowed the suggestion about Barrow to fade into oblivion, cast out by the more public, but I think the less true, charge of plagiarism from Newton. He also saw that he would have to prepare a careful answer if he made one at all, and second thoughts suggested that it would be as well if his postscript was made the matter for further consideration, correction, if necessary, and amplification, before it was sent off. It is to be noted

that the review above mentioned is written anonymously in the third person, but it has been established that its author was Leibniz himself.[14]

There does not seem to be any occasion for further general remarks; particular points of criticism will be alluded to as the translation given below proceeds.

PART I.

§ I.

Full translation of the intended postscript to the letter to James Bernoulli, dated April 1703, from Berlin.

Perhaps[15] you will thing it small-minded of me that I should be irritated with you, your brother, or any one else, if you should have perceived the opportunities for obligation to Barrow, which it was not necessary for me, his contemporary[16] in these discoveries, to have obtained from him.

When I arrived in Paris in the year 1672, I was self-taught as regards geometry,[17] and indeed had little knowledge of the subject, for which I had not the patience to read through the long series of proofs. As a youth I consulted the beginner's Algebra of a certain Lanzius,[18] and afterward that of Clavius;[19] that of Descartes seemed to be more intricate.[20] Nevertheless, it seemed to me, I do not know by what rash confidence in my own ability, that I might become the equal of these if I so desired. I also had the audacity to look through even more profound works, such as the geometry of Cavalieri,[21] and the more pleasant elements of curves of Leotaud,[22] which I happened to come across in Nuremberg, and other things of the kind; from which it is clear that I was now ready to get along without help,[23] for I read them almost as one reads tales of romance.

Meanwhile I was fashioning for myself a kind of geometrical calculus by means of little squares and cubes to express undetermined numbers, being unaware that Descartes and Vieta had worked out the whole matter in a superior manner.[24] In this, I may almost call it, superb ignorance of mathematics, I was then studying history and law; for I had decided to devote myself to the latter.

From mathematics I as it were only sipped those things that were the more pleasant, being especially fond of investigating and inventing machines, for it was at this time that my arithmetical machine[25] was devised. At this time also it happened that Huygens, who I fully believe saw more in me than there really was, with great courtesy brought me a copy recently published of his book on the pendulum.[26] This was for me the beginning or occasion of a more careful study of geometry. While we conversed, he perceived that I had not a correct notion of the center of gravity, and so he briefly described it to me; at the same time he added the information that Dettonville (i. e., Pascal) had worked such things out uncommonly well.[27] Now I, who always had the peculiarity that I was the most teachable of mortals, often cast aside innumerable meditations of mine that were not brought to maturity, when as it were they were swallowed up in the light shed upon them by a few words from some great man, immediately to grasp with avidity the teachings of a mathematician of the highest class; for I quickly saw how great was Huygens. In addition there was the stimulus of shame, in that I appeared to be ignorant with regard to such matters. So I sought a Dettonville from Buotius, a Gregory St. Vincent[28] from the Royal Library, and started to study geometry in earnest. Without delay I examined with delight the "*ductus*" of St. Vincent, and the "*ungulae*"[29] begun by St. Vincent and developed by Pascal, and those sums and sums of sums and solids formed and resolved in various ways; for they afforded me more pleasure than trouble.

I was working upon these when I happened to come across a proof of Dettonville's that was of a supremely easy nature, by which he proved the mensuration of the sphere as given by Archimedes,[30] and showed from the similarity of the triangles EDC and CBK that CK into DE = BC into EC; and hence, by taking BF = CK, that the rectangle AF is equal to the moment[31] of the curve AEC about the axis AB. [Fig. 1.]

The novelty of the reasoning struck me forcibly, for I had not noticed it in the works of Cavalieri.[32] But nothing astonished me so much as the fact that Pascal seemed to have had his eyes obscured by some evil fate; for I saw at a glance that the theorem was a most general one for any kind of curve whatever. Thus, let the perpendiculars not all meet in a point, but let each perpendicular from the curve be transferred to the position of an ordinate to the axis, as

PC or (P)(C) to the position BF or (B)(F); then it is clear that the zone FB(B)(F)F will be equal to the moment of the curve C(C) about the axis.[33] [Fig. 2.]

Straightway I went to Huygens, whom I had not seen again in the meantime. I told him that I had followed out his instructions and that I was now able to do something that Pascal had failed to do. Then I showed him the general theorem for moments of curves. He was struck with wonder and said, "Now, that is the very theorem upon which depend my constructions for finding the area[34] of the surfaces of parabolic, elliptic and hyperbolic conoids; and how these were discovered, neither Roberval nor Bullialdus[35] were ever able to understand." Thus praising my progress, he asked me whether I could not now find the properties of such curves as F(F).

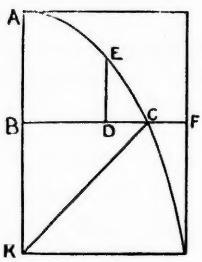


Fig. 1.

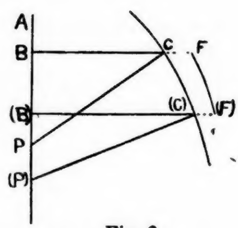


Fig. 2.

When I told him that I had made no investigation in this direction he told me to read the works of Descartes and Slusius,[36] who showed how to form equations for loci; for he said that this idea was a most useful one. Thereupon I examined the Geometry of Descartes and made a close study of Slusius, thus entering the house of geometry truly as it were by the back door. Urged on by the success I met with, and by the great number of results that I obtained, I filled some hundreds of sheets with them in that year. These I divided into two classes of assignables and inassignables. Among assignables I placed everything I obtained by the methods previously used by Cavalieri, Guldinus, Toricelli, Gregory St. Vincent and Pascal, such as sums, sums of sums, transpositions, "ductus," cylinders truncated by a plane, and lastly by the method of the center of gravity; and among inassignables I placed all that I obtained by the use of the triangle which I at that time called "the

characteristic triangle,"[37] and things of the same class, of which Huygens and Wallis seemed to me to have been the originators.

A little later there fell into my hands the Universal Geometry of James Gregory of Scotland,[38] in which I saw the same idea exploited (although obscured by the proofs, which he gave according to the manner of the ancients), and as in Barrow, when his Lectures appeared, in which latter I found the greater part of my theorems anticipated.[39]

However I did not mind this very much, since I saw that these things were perfectly easy to the veriest beginner who had been trained to use them,[40] and because I perceived that there remained much higher matters, which however required a new kind of calculus. Thus I did not think that my Arithmetical Quadrature, although it was received by the French and English with great commendation, was worth being published, as I was loath to waste time over such trifles while the whole ocean was open to me. How matters then proceeded you already know, and as my letters, which the English themselves have published, prove.[41]

§ II.

HISTORY AND ORIGIN OF THE DIFFERENTIAL CALCULUS.

It is an extremely useful thing to have knowledge of the true origins of memorable discoveries, especially those that have been found not by accident but by dint of meditation. It is not so much that thereby history may attribute to each man his own discoveries and that others should be encouraged to earn like commendation, as that the art of making discoveries should be extended by considering noteworthy examples of it.

Among the most renowned discoveries of the times must be considered that of a new kind of mathematical analysis, known by the name of the differential calculus; and of this, even if the essentials are at the present time considered to be sufficiently demonstrated, nevertheless the origin and the method of the discovery are not yet known to the world at large. Its author invented it nearly forty years ago, and nine years later (nearly thirty years ago) published it in a concise form; and from that time it has not only been frequently made known in memoirs,[42] but also has been a method of general employment; while many splendid discoveries

have been made by its assistance, such as have been included in the *Acta Eruditorum,* Leipsic, and also such as have been published in the memoirs of the Royal Academy of Sciences; so that it would seem that a new aspect has been given to mathematical knowledge arising out of its discovery.

Now there never existed any uncertainty as to the name of the true inventor, until recently, in 1712, certain upstarts, either in ignorance of the literature of the times gone by, or through envy, or with some slight hope of gaining notoriety by the discussion, or lastly from obsequious flattery, have set up a rival to him; and by their praise of this rival, the author has suffered no small disparagement in the matter, for the former has been credited with having known far more than is to be found in the subject under discussion. Moreover, in this they acted with considerable shrewdness, in that they put off starting the dispute until those who knew the circumstances, Huygens, Wallis, Tschirnhaus, and others, on whose testimony they could have been refuted, were all dead.[43] Indeed this is one good reason why contemporary prescripts should be introduced as a matter of law; for without any fault or deceit on the part of the responsible party, attacks may be deferred until the evidence with which he might be able to safeguard himself against his opponent had ceased to exist. Moreover, they have changed the whole point of the issue, for in their screed, in which under the title of *Commercium Epistolicum D. Johannis Collinsii* (1712) they have set forth their opinion in such a manner as to give a dubious credit to Leibniz, they have said very little about the calculus; instead, every other page is made up of what they call infinite series. Such things were first given as discoveries by Nicolaus Mercator[44] of Holstein, who obtained them by the process of division, and Newton gave the more general form by extraction of roots.[45] This is certainly a useful discovery, for by it arithmetical approximations are reduced to an analytical reckoning; but it has nothing at all to do with the differential calculus. Moreover, even in this they make use of fallacious reasoning; for whenever this rival works out a quadrature by the addition of the parts by which a figure is gradually increased,[46] at once they hail it as the use of the differential calculus (as for instance on page 15 of the *Commercium*). By the selfsame argument, Kepler (in his *Stereometria Doliorum*),[47] Cavalieri, Fermat, Huygens, and Wallis used the differential calculus; and indeed, of those who dealt with "in-

divisibles" or the "infinitely small," who did not use it? But Huygens, who as a matter of fact had some knowledge of the method of fluxions as far as they are known and used, had the fairness to acknowledge that a new light was shed upon geometry by this calculus, and that knowledge of things beyond the province of that science was wonderfully advanced by its use.

Now it certainly never entered the mind of any one else before Leibniz to institute the notation peculiar to the new calculus by which the imagination is freed from a perpetual reference to diagrams, as was made by Vieta and Descartes in their ordinary or Apollonian geometry; moreover, the more advanced parts pertaining to Archimedean geometry, and to lines which were called "mechanical"[48] by Descartes, were excluded by the latter in his calculus. But now by the calculus of Leibniz the whole of geometry is subjected to analytical computation, and those transcendent lines that Descartes called mechanical are also reduced to equations chosen to suit them, by considering the differences dx, ddx, etc., and the sums that are the inverses of these differences, as functions of the x's; and this, by merely introducing the calculus, whereas before this no other functions were admissible but x, xx, x^3, $\sqrt{x}$, etc., that is to say, powers and roots.[49] Hence it is easy to see that those who expressed these differences by 0, as did Fermat, Descartes, and even that rival, in his *Principia* published in 16—,[50] were by that very fact an extremely long way off from the differential calculus; for in this way neither gradation of the differences nor the differential functions of the several quantities can possibly be made out.

There does not exist anywhere the slightest trace of these methods having been practised by any one before Leibniz.[51] With precisely the same amount of justice as his opponents display in now assigning such discoveries to Newton, any one could equally well assign the geometry of Descartes to Apollonius, who, although he possessed the essential idea of the calculus, yet did not possess the calculus.

For this reason also the new discoveries that were made by the help of the differential calculus were hidden from the followers of Newton's method, nor could they produce anything of real value nor even avoid inaccuracies until they learned the calculus of Leibniz, as is found in the investigation of the catenary as made by David Gregory.[52] But these contentious persons have dared to misuse the name of the English Royal Society, which body took

pains to have it made known that no really definite decision was come to by them; and this is only what is worthy of their reputation for fair dealing, in that one of the two parties was not heard, indeed my friend himself did not know that the Royal Society had undertaken an inquiry into the matter. Else the names of those to whom it had entrusted the report would have been communicated to him,[53] so that they might either be objected to, or equipped for their task. He indeed, astounded not by their arguments but by the fictions that pervaded their attack on his good faith, considered such things unworthy of a reply, knowing as he did that it would be useless to defend his case before those who were unacquainted with this subject (i. e., the great majority of readers); also feeling that those who were skilled in the matter under discussion would readily perceive the injustice of the charge.[54] To this was added the reason that he was absent from home when these reports were circulated by his opponents, and returning home after an interval of two years and being occupied with other business, it was then too late to find and consult the remains of his own past correspondence from which he might refresh his memory about matters that had happened so long ago as forty years previously. For transcripts of very many of the letters once written by him had not been kept; besides those that Wallis found in England and published with his consent in the third volume of his works, Leibniz himself had not very many.

Nevertheless, he did not lack for friends to look after his fair name; and indeed a certain mathematician, one of the first rank of our time[55] well skilled in this branch of learning and perfectly unbiased, whose good-will the opposite party had tried in vain to obtain, plainly stated, giving reasons of his own finding, and let it be known, not altogether with strict justice, that he considered that not only had that rival not invented the calculus, but that in addition he did not understand it to any great extent.[56] Another friend of the inventor[57] published these and other things as well in a short pamphlet, in order to check their base contentions. However it was of greater service to make known the manner and reasoning by which the discoverer arrived at this new kind of calculus; for this indeed has been unknown up till now, even to those perchance, who would like to share in this discovery. Indeed he himself had decided to explain it, and to give an account of the course of his researches in analysis partly from memory and partly from extant

writings and remains of old manuscripts, and in this manner to illustrate in due form in a little book the history of this higher learning and the method of its discovery. But since at the time this was found to be impossible owing to the necessities of other business, he allowed this short statement of part of what there was to tell upon the matter to be published in the meantime by a friend who knew all about it,[58] so that in some measure public curiosity should be satisfied.

The author of this new analysis, in the first flower of his youth, added to the study of history and jurisprudence other more profound reflections for which he had a natural inclination. Among the latter he took a keen delight in the properties and combinations of numbers; indeed, in 1666 he published an essay, *De Arte Combinatoria,* afterward reprinted without his sanction. Also, while still a boy, when studying logic he perceived that the ultimate analysis of truths that depended on reasoning reduced to two things, definitions and identical truths, and that these alone of the essentials were primitive and undemonstrable. When it was stated in contradiction that identical truths were useless and nugatory, he gave illustrative proofs to the contrary. Among these he gave a demonstration that that mighty axiom, "The whole is greater than its part," could be proved by a syllogism of which the major term was a definition and the minor term an identity.[59] For if one of two things is equal to a part of another the former is called the less, and the later the greater; and this is to be taken as the definition. Now, if to this definition there be added the following identical and undemonstrable axiom, "Every thing possessed of magnitude is equal to itself," i. e., $A = A$, then we have the syllogism:

Whatever is equal to a part of another, is less than that other:
(by the definition)

But the part is equal to a part of the whole:
(i. e., to itself, by identity)

Hence the part is less than the whole. Q. E. D.

As an immediate consequence of this he observed that from the identity $A = A$, or at any rate from its equivalent, $A - A = 0$, as may be seen at a glance by straightforward reduction, the following very pretty property of differences arises, namely:

$$A \quad \underbrace{-A+B}_{+\ L} \quad \underbrace{-B+C}_{+\ M} \quad \underbrace{-C+D}_{+\ N} \quad \underbrace{-D+E}_{+\ P} \quad -E = 0$$

If now A, B, C, D, E are supposed to be quantities that continually increase in magnitude, and the differences between successive terms are denoted by L, M, N, P, it will then follow that

$$A + L + M + N + P = 0,$$
$$\text{i. e.,} \qquad L + M + N + P = E - A;$$

that is, the sums of the differences between successive terms, no matter how great their number, will be equal to the difference between the terms at the beginning and the end of the series.[60] For example, in place of A, B, C, D, E, let us take the squares, 0, 1, 4, 9, 16, 25, and instead of the differences given above, the odd numbers, 1, 3, 5, 7, 9, will be disclosed; thus

$$\begin{array}{ccccccccccc} 0 & & 1 & & 4 & & 9 & & 15 & & 25 \\ & 1 & & 3 & & 5 & & 7 & & 9 & \end{array}$$

From which is evident that

$$1 + 3 + 5 + 7 + 9 = 25 - 0 = 25,$$
$$\text{and} \qquad 3 + 5 + 7 + 9 = 25 - 1 = 24;$$

and the same will hold good whatever the number of terms or the differences may be, or whatever numbers are taken as the first and last terms.

Delighted by this easy and elegant theorem, our young friend considered a large number of numerical series, and also proceeded to the second differences or differences of the differences,[61] the third differences or the differences between the differences of the differences, and so on. He also observed that for the natural numbers, i. e., the numbers in order proceeding from 0, the second differences vanished, as also did the third differences for the squares, the fourth differences for the cubes, the fifth for the biquadrates, the sixth for the surdesolids,[62] and so on; also that the first differences for the natural numbers were constant and equal to 1; the second differences for the square, 1.2, or 2; the third for the cubes, 1.2.3, or 6; the fourth for the biquadrates, 1.2.3.4, or 24; the fifth for the surdesolids, 1.2.3.4.5, or 120, and so on. These things it is admitted had been previously noted by others,

but they were new to him, and by their easiness and elegance were in themselves an inducement to further advances. But especially he considered what he called "combinatory numbers," such as are usually tabulated as in the margin. Here a preceding series, either horizontal or vertical, always contains the first differences of the series immediately following it, the second differences of the one next after that, the third differences of the third, and so on. Also, each series, either horizontal or vertical contains the sums of the series immediately preceding it, the sums of the sums or the second sums of the series next before that, the third sums of the third, and so on. But, to give something not yet common knowledge, he also brought to light certain general theorems on differences and sums, such as the following. In the series, *a, b, c, d, e,* etc., where the terms continually decrease without limit we have

1	1	1	1	1	1
1	2	3	4	5	6
1	3	6	10	15	21
1	4	10	20	35	56
1	5	15	35	70	126
1	6	21	56	126	252
1	7	28	84	210	462

Terms	a	b	c	d	e	etc.
1st diff.	f	g	h	i	k	etc.
2nd diff.	l	m	n	o	p	etc.
3rd diff.	q	r	s	t	u	etc.
4th diff.	β	γ	δ	ϵ	θ	etc.
etc.	γ	μ	ν	ρ	υ	etc.

Taking a as the first term, and ω as the last, he found

$$\begin{aligned}
a-\omega &= 1f + 1g + 1h + 1i + 1k + \text{etc.}\\
a-\omega &= 1l + 2m + 3n + 4o + 5p + \text{etc.}\\
a-\omega &= 1q + 3r + 6s + 10t + 15u + \text{etc.}\\
a-\omega &= 1\beta + 4\gamma + 10\delta + 20\epsilon + 35\theta + \text{etc.}\\
&\text{etc.}
\end{aligned}$$

Again we have[63]

$$a-\omega=\left\{\begin{array}{l}
+1f\\
+1f-1l\\
+1f-2l+1q\\
+1f-3l+3q-1\beta\\
+1f-4l+6q-4\beta+1\lambda\\
\qquad \text{etc. etc. etc,}
\end{array}\right.$$

Hence, adopting a notation invented by him at a later date, and denoting any term of the series generally by y (in which case $a = y$ as well), we may call the first difference dy, the second ddy, the third d^3y, the fourth d^4y; and calling any term of another of the series x, we may denote the sum of its terms by $\int x$, the sum of their sums or their second sum by $\int\int x$, the third sum by $\int^3 x$, and the fourth sum by $\int^4 x$. Hence, supposing that

$$1 + 1 + 1 + 1 + 1 + \text{etc.} = x,$$

or that x represents the natural numbers, for which $dx = 1$, then

$$\begin{aligned} 1 + 3 + 6 + 10 + \text{etc.} &= \int x, \\ 1 + 4 + 10 + 20 + \text{etc.} &= \int\int x, \\ 1 + 5 + 15 + 35 + \text{etc.} &= \int^3 x, \end{aligned}$$

and so on. Finally it follows that

$$y - \omega = dy . x - ddy . \int x + d^3y . \int\int x - d^4y . \int^3 x + \text{etc.};$$

and this is equal to y, if we suppose that the series is continued to infinity, or that ω becomes zero. Hence also follows the sum of the series itself, and we have

$$\int y = yx - dy . \int x + ddy . \int\int x - d^3y . \int^3 x + \text{etc.}$$[64]

These two like theorems possess the uncommon property that they are equally true in either differential calculus, the numerical or the infinitesimal; of the distinction between them we will speak later.[65]

However, the application of numerical truths to geometry, as well as the consideration of infinite series, was at that time at all events unknown to our young friend, and he was content with the satisfaction of having observed such things in series of numbers. Nor did he then, except for the most ordinary practical rules, know anything about geometry;[66] he had scarcely even considered Euclid with anything like proper attention, being fully occupied with other studies. However, by chance he came across the delightful contemplation of curves by Leotaud, in which the author deals with the quadrature of lunules, and Cavalieri's geometry of indivisibles;[67] having given these some slight consideration, he was delighted

with the facility of their methods. However, at that time he was in no mind to go fully in these more profound parts of mathematics, although just afterwards he gave attention to the study of physics and practical mechanics, as may be understood from his essay that he published on the *Hypothesis of Physics*.[68]

He then became a member of the Revision Council[69] of the Most Noble the Elector of Mainz; later, having obtained permission from this Most Gracious and Puissant Prince (for he had taken our young friend into his personal service when he was about to leave[70] and go further afield) to continue his travels, he set out for Paris in the year 1672. There he became acquainted with that genius Christian Huygens, to whose example and precepts he always declared that he owed his introduction to higher mathematics. At that time it so happened that Huygens was engaged on his work with regard to the pendulum. When Huygens brought our young friend a copy of this work as a present and in the course of conversation discussed the nature of the center of gravity, which our young friend did not know very much about, the former explained to him shortly what sort of thing it was and how it could be investigated.[71] This roused our young friend from his lethargy, for he looked upon it as something of a disgrace that he should be ignorant of such matters.[72]

Now it was impossible for him to find time for such studies just then; for almost immediately, at the close of the year, he crossed the Channel to England in the suite of the envoy from Mainz, and stayed there for a few weeks with the envoy. Having been introduced by Henry Oldenburg, at that time secretary to the Royal Society, he was elected a member of that illustrious body. He did not however at that time discuss geometry with any one (in truth at that time he was quite one of the common herd as regards this subject); he did not on the other hand neglect chemistry, consulting that excellent man Robert Boyle on several occasions. He also came across Pell accidentally, and he described to him certain of his own observations on numbers; and Pell told him that they were not new, but that it had been recently made known by Nicolaus Mercator, in his *Hyperbolae Quadratura,* that the differences of the powers of the natural numbers, when taken continuously, finally vanished; this made Leibniz obtain the work of Nicolaus Mercator.[73] At that time he did not become acquainted with Collins; and, although he conversed with Oldenburg on literary

matters, on physics and mechanics, he did not exchange with him even one little word on higher geomery, much less on the series of Newton. Indeed, that he was almost a stranger to these subjects, except perhaps in the properties of numbers, even that he had not paid very much attention to them, is shown well enough by the letters which he exchanged with Oldenburg, which have been lately published by his opponents. The same fact will appear clearly from those which they say have been preserved in England; but they suppressed them,[74] I firmly believe, because it would be quite clear from them that up to then there had been no correspondence between him and Oldenburg on matters geometrical. Nevertheless, they would have it credited (not indeed with the slightest evidence brought forward in favor of the supposition) that certain results obtained by Collins, Gregory and Newton, which were in the possession of Oldenburg, were communicated by him to Leibniz.

On his return from England to France in the year 1673,[75] having meanwhile satisfactorily performed his work for the Most Noble Elector of Mainz, he still by his favor remained in the service of Mainz; but his time being left more free, at the instigation of Huygens he began to work at Cartesian analysis (which aforetime had been beyond him),[76] and in order to obtain an insight into the geometry of quadratures he consulted the *Synopsis Geometriae* of Honoratus Fabri, Gregory St. Vincent, and a little book by Dettonville (i. e., Pascal).[77] Later on from one example given by Dettonville, a light suddenly burst upon him, which strange to say Pascal himself had not perceived in it. For when he proves the theorem of Archimedes for measuring the surface of a sphere or parts of it, he used a method in which the whole surface of the solid formed by a rotation round any axis can be reduced to an equivalent plane figure. From it our young friend made out for himself the following general theorem.[78]

Portions of a straight line normal to a curve, intercepted between the curve and an axis, when taken in order and applied at right angles to the axis give rise to a figure equivalent to the moment of the curve about the axis.[79]

When he showed this to Huygens the latter praised him highly and confessed to him that by the help of this very theorem he had found the surface of parabolic conoids and others of the same sort, stated without proof many years before in his work on the pendulum clock. Our young friend, stimulated by this and pondering

on the fertility of this point of view, since previously he had considered infinitely small things such as the intervals between the ordinates in the method of Cavalieri and such only, studied the triangle ${}_1Y\,D\,{}_2Y$, which he called the Characteristic Triangle,[80] whose sides $D\,{}_1Y$, $D\,{}_2Y$ are respectively equal to ${}_1X\,{}_2X$, ${}_1Z\,{}_2Z$,[81] parts of the coordinates or coabscissae AX, AZ, and its third side ${}_1Y\,{}_2Y$ a part of the tangent TV, produced if necessary.

Even though this triangle is indefinite (being infinitely small), yet he perceived that it was always possible to find definite triangles similar to it. For, suppose that AXX, AZZ are two straight lines at right angles, and AX, AZ the coabscissae, YX, YZ the coordi-

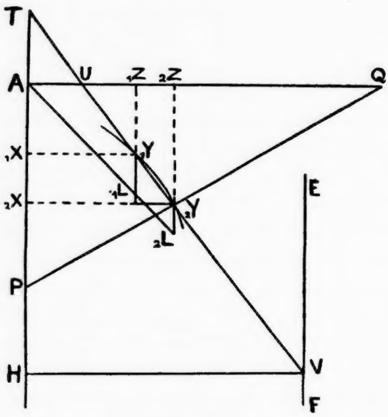


Fig. 3.

nates, TUV the tangent, PYQ the perpendicular, XT, ZU the subtangents, XP, ZQ the subnormals; and lastly let EF be drawn parallel to the axis AX; let the tangent TY meet EF in V, and from V draw VH perpendicular to the axis. Then the triangles ${}_1YD\,{}_2Y$, TXY, YZU, TAU, YXP, QZY, QAP, THV, and as many more of the sort as you like, are all similar. For example, from the similar triangles ${}_1YD\,{}_2Y$, ${}_2Y\,{}_2XP$, we have $P\,{}_2Y \cdot {}_1YD = {}_2Y\,{}_2X \cdot {}_2Y\,{}_1Y$; that is, the rectangle contained by the $P\,{}_2Y$ and ${}_1YD$ (or the element of the axis, ${}_1X\,{}_2X$) is equal to the rectangle contained by the ordinate ${}_2Y\,{}_2X$ and the element of the curve, ${}_1Y\,{}_2Y$, that is, to the moment of the element of the curve about the axis. Hence the

whole moment of the curve is obtained by forming the sum of these perpendiculars to the axis.

Also, on account of the similar triangles ${}_1YD\,{}_2Y$, THV, we have ${}_1Y\,{}_2Y : {}_2YD = TV = VH$, or $VH.{}_1Y\,{}_2Y = TV.{}_2YD$; that is, the rectangle contained by the constant length VH and the element of the curve, ${}_1Y\,{}_2Y$, is equal to the rectangle contained by TV and ${}_2YD$, or the element of the coabscissa, ${}_1Z\,{}_2Z$. Hence the plane figure produced by applying the lines TV in order at right angles to AZ is equal to the rectangle contained by the curve when straightened out and the constant length HV.

Again, from the similar triangles ${}_1YD\,{}_2Y$, ${}_2Y\,{}_2XP$, we have ${}_1YD : D\,{}_2Y = {}_2Y\,{}_2X : {}_2XP$, and thus ${}_2XP.\,{}_1YD = {}_2Y\,{}_2X.D\,{}_2Y$, or the sum of the subnormals ${}_2XP$, taken in order and applied to the axis, either to ${}_1YD$ or to ${}_1X\,{}_2X$ and their elements ${}_2YD$, taken in order. But straight lines that continually increase from zero, when each is multiplied by its element of increase, form altogether a triangle. Let then AZ always be equal to ZL, then we get the right-angled triangle AZL, which is half the square on AZ; and thus the figure that is produced by taking the subnormals in order and applying them perpendicular to the axis will be always equal to half the square on the ordinate. Thus, to find the area of a given figure, another figure is sought such that its subnormals are respectively equal to the ordinates of the given figure, and then this second figure is the quadratrix of the given one; and thus from this extremely elegant consideration we obtain the reduction of the areas of surfaces described by rotation[82] to plane quadratures, as well as the rectification of curves; at the same time we can reduce these quadratures of figures to an inverse problem of tangents. From these results,[83] our young friend wrote down a large collection of theorems (among which in truth there were many that were lacking in elegance) of two kinds. For in some of them only definite magnitudes were dealt with, after the manner not only of Cavalieri, Fermat, Honoratus Fabri, but also of Gregory St. Vincent, Guldinus, and Dettonville; others truly depended on infinitely small magnitudes, and advanced to a much greater extent. But later our young friend did not not trouble to go on with these matters, when he noticed that the same method can be brought into use and perfected by not only Huygens, Wallis, Van Huraet, and Neil, but also by James Gregory and Barrow. However it did not seem to me to be altogether useless to explain at this junc-

ture, as is plain from what I have given,[84] the steps by which he attained to greater things, and also the manner in which, as if led by the hand, those who are at present but beginners[85] with regard to the more abstruse parts of geometry may hope to rise to greater heights.

Now Leibniz worked these things out at Paris in the year 1673 and part of 1674. But in the year 1674 (so much it is possible to state definitely), he came upon the well-known arithmetical teragonism;[86] and it will be worth while to explain how this was accomplished. He once happened to have occasion to break up an area into triangles formed by a number of straight lines meeting in a

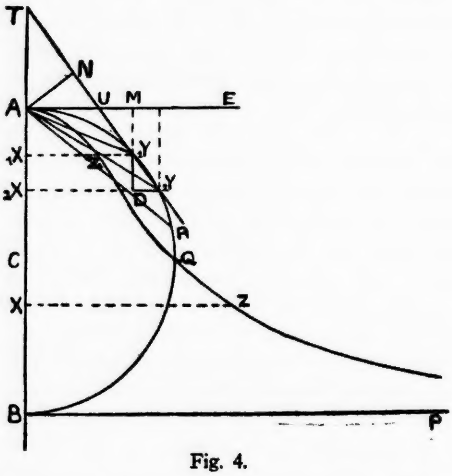


Fig. 4.

point, and he perceived that something new could be readily obtained from it.[87]

In Fig. 4, let any number of straight lines, AY, be drawn to the curve AYR, and let any axis AC be drawn, and AE a normal or coaxis to it; and let the tangent at Y to the curve cut them in T and U. From A draw AN perpendicular to the tangent; then it is plain that the elementary triangle $A\,{}_1Y\,{}_2Y$ is equal to half the rectangle contained by the element of the curve ${}_1Y\,{}_2Y$ and AN. Now draw the characteristic triangle mentioned above, ${}_1YD\,{}_2Y$, of which the hypotenuse is a portion of the tangent or the element of the arc, and the sides are parallel to the axis and the coaxis. It

is then plain from the similar triangles ANU, ${}_1YD_2Y$, that ${}_1Y_2Y : {}_1YD = AU : AN$, or $AU . {}_1YD$ or $AU . {}_1X_2X$ is equal to $AN . {}_1Y_2Y$, and this, as has been already shown, is equal to double the triangle A_1Y_2Y. Thus if every AU is supposed to be transferred to XY, and taken in it as AZ,[88] then the trilinear space AXZA so formed will be equal to twice the segment AYA,[89] included between the straight line AY and the arc AY. In this way are obtained what he called the figures of segments or the proportionals of a segment. A similar method holds good for the case in which the point is not taken on the curve, and in this manner he obtained the proportional trilinear figures for sectors cut off by lines meeting in the point; and even when the straight lines had their extremities not in a line but in a curve (which one after the other they touched), none the less on that account were useful theorems made out.[90] But this is not a fit occasion to follow out such matters; it is sufficient for our purpose to consider the figures of segments, and that too only for the circle. In this case, if the point A is taken at the beginning of the quadrant AYQ, the curve AZQZ will cut the circle at Q, the other end of the quadrant, and thence descending will be asymptotic to the base BP (drawn at right angles to the diameter at its other end B); and, although extending to infinity, the whole figure, included between the diameter AB, the base BP...., and the curve AZQZ.... asymptotic to it, will be equal to the circle on AB as diameter.

But to come to the matter under discussion, take the radius as unity, put AX or UZ = x, and AU or AZ = z, then we have $x = 2zz :, 1 + zz$;[91] and the sum of all the x's applied to AU, which at the present time we call $\int x\,dz$, is the trilinear figure AUZA, which is the complement of the trilinear figure AXZA, and this has been shown to be double the circular segment.

The author obtained the same result by the method of transmutations, of which he sent an account to England.[92] It is required to form the sum of all the ordinates $\sqrt{(1 - xx)} = y$; suppose $y = \pm 1 \mp xz$, from which $x = 2z :, 1 + zz$, and $y = \pm zz \mp 1, :, zz + 1$; and thus again all that remains to be done is the summation of rationals.

This seemed to him to be a new and elegant method, as it did to Newton also, but it must be acknowledged that it is not of universal application. Moreover it is evident that in this way the arc may be obtained from the sine, and other things of

the same kind, but indirectly. So when later he heard that these things had been derived in a direct manner by Newton with the help of root-extractions,[93] he was desirous of getting a knowledge of the matter.

From the above it was at once apparent that, using the method by which Nicolaus Mercator had given the arithmetical tetragonism of the hyperbola by means of an infinite series, that of the circle might also be given, though not so symmetrically, by dividing by $1+zz$, in the same way that the former had divided by $1+z$. The author, however, soon found a general theorem for the area of any central conic. Namely, the sector included by the arc of a conic section, starting from the vertex, and two straight lines joining its ends to the center, is equal to the rectangle contained by the semi-transverse axis and a straight line of length

$$t \pm \frac{1}{3}t^3 + \frac{1}{5}t^5 \pm \frac{1}{7}t^7 + \ldots\ldots,$$ [94]

where t is the portion of the tangent at the vertex intercepted between the vertex and the tangent at the other extremity of the arc, and unity is the square on the semi-conjugate axis or the rectangle contained by the halves of the latus-rectum and the transverse axis, and $\pm$ is to be taken to mean + for the hyperbola and − for the circle or the ellipse. Hence if the square of the diameter is taken to be unity, then the area of the circle is

$$\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11} + \text{etc.}$$

When our friend showed this to Huygens, together with a proof of it, the latter praised it very highly, and when he returned the dissertation said, in the letter that accompanied it, that it would be a discovery always to be remembered among mathematicians, and that in it the hope was born that at some time it might be possible that the general solution should be obtained either by exhibiting its true value or by proving the impossibility of expressing it in recognized numbers.[95] There is no doubt that neither he nor the discoverer, nor yet any one else in Paris, had heard anything at all by report concerning the expression of the area of a circle by means of an infinite series of rationals (such as afterward it became known had been worked out by Newton and

Gregory). Certainly Huygens did not, as is evident from the short letter from him that I give herewith.[96]... Thus Huygens believed that it was now proved for the first time that the area of a circle was exactly equal to a series of rational quantities. Leibniz (relying on the opinion of Huygens, who was well versed in such matters), believed the same thing and so wrote those two letters to Oldenburg in 1674, which his opponents have published, in which he announces it as a new discovery;[97] indeed he went so far as to say that he, before all others, had found the magnitude of the circle expressed as a series of rational numbers, as had already been done in the case of the hyperbola.[98] Now, if Oldenburg had already communicated to him during his stay in London the series of Newton and Gregory,[99] it would have been the height of impudence for him to have dared to write in this way to Oldenburg; and either forgetfulness or collusion on the part of Oldenburg in not charging him with the deceit. For these opponents publish the reply of Oldenburg, in which he merely points out (he says "I do not wish you to be unaware....") that similar series had been noted by Gregory and Newton; and these things also he communicated in the year following in a letter (which they publish) written in the month of April.[100] From which it can be seen that they are blinded with envy or shameless with spite who dare to pretend that Oldenburg had already communicated those things to him in the preceding year. Yet there may be some blindness in their spite, because they do not see that they publish things by which their lying statements are refuted, nor that it would have been far better to have suppressed these letters between him and Oldenburg, as they have done in the case of others, either wholly or in part. Besides, from this time onwards he begins to correspond with Oldenburg about geometry; that is, from the time when he, who up till then had been but a beginner in this subject, first found out anything that he considered worthy to be communicated; and former letters written from Paris on March 30, April 26, May 24, and June 8, in the year 1673, which they say they have at hand but suppress, together with the replies of Oldenburg, must undoubtedly have dealt with other matters and have nothing in them to render those fictitious communications from Oldenburg the more deserving of belief. Again, when our young friend heard that Newton and Gregory had discovered their series by the extraction of roots,[101] he acknowledged that this was new to him, nor at first did he understand it very

much; and he confessed as much quite frankly and asked for information on certain points, especially for the case in which reciprocal series were sought, by means of which from one infinite series the root was extracted by means of another infinite series. And from this also it is evident that what his opponents assert, that Oldenburg communicated the writings of Newton to him, is false; for if that were the truth, there would have been no need to ask for further information. On the other hand, when he began to develop his differential calculus, he was convinced that the new method was much more universal for finding infinite series without root-extractions, and adapted not only for ordinary quantities but for transcendent quantities as well, by assuming that the series required was given; and he used this method to complete his short essay on the arithmetical quadrature; in this he also included other series that he had discovered, such as an expression for the arc in terms of the sine or the complement of the sine, and conversely he showed how, by this same method, to find the sine or cosine when the arc was given.[102] This too is the reason why later he stood in no need of other methods than his own; and finally, he published his own new way of obtaining series in the *Acta Eruditorum.* Moreover, as it was at this time, just after he had published the essay on the Arithmetical Quadrature in Paris, that he was recalled to Germany, having perfected the technique of the new calculus he paid less attention to the former methods.

Now it is to be shown how, little by little, our friend arrived at the new kind of notation that he called the differential calculus. In the year 1672, while conversing with Huygens on the properties of numbers, the latter propounded to him this problem:[103]

To find the sum of a decreasing series of fractions, of which the numerators are all unity and the denominators are the triangular numbers; of which he said that he had found the sum among the contributions of Hudde on the estimation of probability. Leibniz found the sum to be 2, which agreed with that given by Huygens. While doing this he found the sums of a number of arithmetical series of the same kind in which the numbers are any combinatory numbers whatever, and communicated the results to Oldenburg in February 1673, as his opponents have stated. When later he saw the Arithmetical Triangle of Pascal, he formed on the same plan his own Harmonic Triangle.

Arithmetical Triangle

in which the fundamental series is an arithmetical progression

$$1,\ 2,\ 3,\ 4,\ 5,\ 6,\ 7,\ \ldots$$

$$\begin{array}{ccccccccccccccc}
&&&&&&&1&&&&&&&\\
&&&&&&1&&1&&&&&&\\
&&&&&1&&2&&1&&&&&\\
&&&&1&&3&&3&&1&&&&\\
&&&1&&4&&6&&4&&1&&&\\
&&1&&5&&10&&10&&5&&1&&\\
&1&&6&&15&&20&&15&&6&&1&\\
1&&7&&21&&35&&35&&21&&7&&1
\end{array}$$

Harmonic Triangle[104]

in which the fundamental series is a harmonical progression

$$\begin{array}{ccccccccccccc}
&&&&&&\frac{1}{1}&&&&&&\\
&&&&&\frac{1}{2}&&\frac{1}{2}&&&&&\\
&&&&\frac{1}{3}&&\frac{1}{6}&&\frac{1}{3}&&&&\\
&&&\frac{1}{4}&&\frac{1}{12}&&\frac{1}{12}&&\frac{1}{4}&&&\\
&&\frac{1}{5}&&\frac{1}{20}&&\frac{1}{30}&&\frac{1}{20}&&\frac{1}{5}&&\\
&\frac{1}{6}&&\frac{1}{30}&&\frac{1}{60}&&\frac{1}{60}&&\frac{1}{30}&&\frac{1}{6}&\\
\frac{1}{7}&&\frac{1}{42}&&\frac{1}{105}&&\frac{1}{140}&&\frac{1}{105}&&\frac{1}{42}&&\frac{1}{7}
\end{array}$$

where, if the denominators of any series descending obliquely to infinity or of any parallel finite series, are each divided by the term that corresponds in the first series, the combinatory numbers are produced, namely those that are contained in the arithmetical triangle. Moreover this property is common to either triangle, namely, that the oblique series are the sum- and difference-series of one another. In the Arithmetical Triangle any given series is the sum-series of the series that immediately precedes it, and the difference-

series of the one that follows it; in the Harmonic Triangle, on the other hand, each series is the sum-series of the series following it, and the difference-series of the series that precedes it. From which it follows that

$$\frac{1}{1}+\frac{1}{2}+\frac{1}{3}+\frac{1}{4}+\frac{1}{5}+\frac{1}{6}+\frac{1}{7}+\text{ etc.}=\frac{1}{0}$$

$$\frac{1}{1}+\frac{1}{3}+\frac{1}{6}+\frac{1}{10}+\frac{1}{15}+\frac{1}{21}+\frac{1}{28}+\text{ etc.}=\frac{2}{1}$$

$$\frac{1}{1}+\frac{1}{4}+\frac{1}{10}+\frac{1}{20}+\frac{1}{35}+\frac{1}{56}+\frac{1}{84}+\text{ etc.}=\frac{3}{2}$$

$$\frac{1}{1}+\frac{1}{5}+\frac{1}{15}+\frac{1}{35}+\frac{1}{70}+\frac{1}{126}+\frac{1}{210}+\text{ etc.}=\frac{4}{3}$$

and so on.

Now he had found out these things before he had turned to Cartesian analysis; but when he had had his thoughts directed to this, he considered that any term of a series could in most cases be denoted by some general notation, by which it might be referred to some simple series. For instance, if the general term of the series of natural numbers is denoted by x, then the general term of the series of squares would be x^2, that of the cubes would be x^3, and so on. Any triangular number, such as 0, 1, 3, 6, 10, would be

$$\frac{x.x+1}{1.2} \text{ or } \frac{xx+x}{2},$$

any pyramidal number, such as 0, 1, 4, 10, 20, etc., would be

$$\frac{x.x+1.x+2}{1.2.3} \text{ or } \frac{x^3+3xx+2x}{6},$$

and so on.

From this it was possible to obtain the difference-series of a given series, and in some cases its sum as well, when it was expressed numerically. For instance, the square is xx, the next greater square is $xx+2x+1$, and the difference of these is $2x+1$; i. e., the series of odd numbers is the difference-series for the series of squares. For, if x is 0, 1, 2, 3, 4, etc., then $2x+1$ is 1, 3, 5, 7, 9. In the same way the difference between x^3 and $x^3+3xx+3x+1$ is $3xx+3x+1$, and thus the latter is the general term of the difference-series

for the series of cubes. Further, if the value of the general term can thus be expressed by means of a variable x so that the variable does not enter into a denominator or an exponent, he perceived that he could always find the sum-series of the given series. For instance, to find the sum of the squares, since it is plain that the variable cannot be raised to a higher degree than the cube, he supposed its general term z to be

$$z = lx^3 + mxx + nx, \text{ where } dz \text{ has to be } xx;$$

we have $dz = l\,d(x^3) + m\,d(xx) + n$, (where dx is taken $= 1$); now $d(x^3) = 3xx + 3x + 1$, and $d(xx) = 2x + 1$, as already found; hence

$$dz = 3lxx + 3lx + l + 2mx + m + n \simeq xx;$$ [105]

therefore $l = \frac{1}{3}$, $m = -\frac{1}{2}$, and $\frac{1}{3} - \frac{1}{2} + n = 0$, or $n = \frac{1}{6}$;

and the general term of the sum-series for the squares is

$$\frac{1}{3}x^3 - \frac{1}{2}xx + \frac{1}{6}x \text{ or } 2x^3 - 3xx + x, : 6.$$ [106]

As an example, if it is desired to find the sum of the first nine or ten squares, i. e., from 1 to 81 or from 1 to 100, take for x the values 10 or 11, the numbers next greater than the root of the last square, and $2x^3 - 3xx + x, : 6$ will be $2000 - 300 + 10, : 6 = 285$, or $2.1331 - 3.121 + 11, : 6 = 385$. Nor is it much more difficult with this formula to sum the first 100 or 1000 squares. The same method holds good for any powers of the natural numbers or for expressions which are made up from such powers, so that it is always possible to sum as many terms as we please of such series by a formula. But our friend saw that it was not always easy to proceed in the same way when the variable entered into the denominator, as it was always possible to find the sum of a numerical series; however, on following up this same analytical method, he found in general, and published the result in the *Acta Eruditorum,* that a sum-series could always be found, or the matter be reduced to finding the sum of a number of fractional terms such as $1/x$, $1/xx$, $1/x^3$, etc, which at any rate, if the number of terms taken is finite, can be summed, though hardly in a short way (as by a formula); but if it is a question of an infinite number of terms, then terms such as $1/x$ cannot be summed at all, because the total of an infinite number of terms of such a series is an infinite quantity,

but that of an infinite number of terms such as $1/xx$, $1/x^3$, etc., make a finite quantity, which nevertheless could not up till now be summed, except by taking quadratures. So, in the year 1682, in the month of February, he noted in the *Acta Eruditorum* that if the numbers 1.3, 3.5, 5.7, 7.9, 9.11, etc., or 3, 15, 35, 63, 99, etc., are taken, and from them is formed the series of fractions

$$\frac{1}{3}+\frac{1}{15}+\frac{1}{35}+\frac{1}{63}+\frac{1}{99}+\text{etc.},$$

then the sum of this series continued to infinity is nothing else but $\frac{1}{2}$; while, if every other fraction is left out, $\frac{1}{3}+\frac{1}{35}+\frac{1}{99}+\text{etc.}$ expresses the magnitude of a semicircle of which the square on the diameter is represented by 1.[107]

Thus, suppose $x=1, 2, 3,$ etc.[108] Then the general term of

$$\frac{1}{3}+\frac{1}{15}+\frac{1}{35}+\frac{1}{63}+\text{etc. is } \frac{1}{4xx+8x+3};$$

it is required to find the general term of the sum-series.

Let us try whether it can have the form $e/(bx+c)$, the reasoning being very simple; then we shall have

$$\frac{e}{bx+c}-\frac{e}{bx+b+c}=\frac{eb}{bbxx+bbx+bc+2bcx+cc}\simeq\frac{1}{4xx+8x+3};$$

hence, equating coefficients in these two formulas, we have

$$b=2,\ eb=1,\text{ or } e=\tfrac{1}{2},$$

$$bb+2bc=8,\text{ or } 4+4c=8,\text{ or } c=1;$$

and finally we should have also $bc+cc=3$, which is the case. Hence the general term of the sum-series is $(1:2)/(2x+1)$ or $1/(4x+2)$, and these numbers of the form $4x+2$ are the doubles of the odd numbers. Finally he gave a method for applying the differential calculus to numerical series when the variable entered into the exponent, as in a geometrical progression, where, taking any radix b the term is b^x, where x stands for a natural number. The terms of the differential series will be $b^{x+1}-b^x$, or $b^x(b-1)$; and from this it is plain that the differential series of the given geometrical series is also a geometrical series proportional to the given series. Thus the sum of a geometrical series may be obtained.

But our young friend quickly observed that the differential calculus could be employed with diagrams in an even more wonderfully simple manner than it was with numbers, because with diagrams the differences were not comparable with the things which differed; and as often as they were connected together by addition or subtraction, being incomparable with one another, the less vanished in comparison with the greater; and thus irrationals could be differentiated no less easily than surds, and also, by the aid of logarithms, so could exponentials. Moreover, he observed that the infinitely small lines occurring in diagrams were nothing else but the momentaneous differences of the variable lines. Also, in the same way as quantities hitherto considered by analytical mathematicians had their functions such as powers and roots, so also such quantities as were variable had new functions, namely, differences. Also, that as hitherto we had x, xx, x^3, etc., y, yy, y^3, etc., so now it was possible to have dx, ddx, d^3x, etc., dy, ddy, d^3y, and so forth. In the same way, that it was possible to express curves, which Descartes had excluded as being "mechanical," by equations of position, and to apply the calculus to them and thus to free the mind from a perpetual reference to diagrams. In the applications of the differential calculus to geometry, differentiations of the first degree were equivalent to nothing else but the finding of tangents, differentiations of the second degree to the finding of osculating circles (the use of which was introduced by our friend); and that it was possible to go on in the same fashion. Nor were these things only of service for tangents and quadratures, but for all kinds of problems and theorems in whch the differences were intermingled with integral terms (as that brilliant mathematician Bernoulli called them), such as are used in physico-mechanical problems.

Thus it follows generally that if any series of numbers or lines of a figure have a property that depends on two, three or more consecutive terms, it can be expressed by an equation involving differences of the first, second, third, or higher degree. Moreover, he discovered general theorems for any degree of the differences, just as we have had theorems of any degree, and he made out the remarkable analogy between powers and differences published in the *Miscellania Berolinensia.*

If his rival had known of these matters, he would not have used dots to denote the degrees of the differences,[109] which are useless for expressing the general degree of the differences, but

would have used the symbol d given by our friend or something similar, for then d^e can express the degree of the difference in general. Besides everything which was once referred to figures, can now be expressed by the calculus.

For $\sqrt{(dxdx+dydy)}$[110] was the element of the arc of a curve, ydx was the element of its area; and from that it is immediately evident that $\int y\,dx$ and $\int x\,dy$ are the complements of one another, since $d(xy)=x\,dy+y\,dx$, or conversely, $xy=\int x\,dy+\int y\,dx$, however these figures vary from time to time; and from this, since $xyz=\int xy\,dz+\int xz\,dy+\int yz\,dx$, three solids are also given that are complementary, every two to the third. Nor is there any need for him to have known those theorems which we deduced above from the characteristic triangle; for example, the moment of a curve about the axis is sufficiently expressed by $\int x\sqrt{(dxdx+dydy)}$. Also what Gregory St. Vincent has concerning *ductus*, what he or

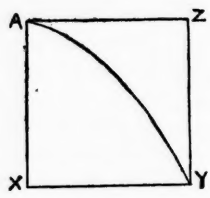


Fig. 5.

Pascal had concerning *ungulae* and *cunei*,[111] every one of these is immediately deduced from a calculus such as this. Thus Leibniz saw with delight those discoveries that he had applauded in others obtained by himself, and thereupon he left off studying them at all closely, because all of them were contained in a calculus such as his.

For example, the moment of the figure AXYA (Fig. 5) about the axis is $\frac{1}{2}\int yy\,dx$, the moment of the figure about the tangent at the vertex is $\int xy\,dx$, the moment of the complementary trilinear figure AZYA about the tangent at the vertex is $\frac{1}{2}\int xx\,dy$. Now these two last moments taken together yield the moment of the circumscribed rectangle AXYZ about the tangent at the vertex, and are complementary to one another.

However, the calculus also shows this without reference to any figure, for $\frac{1}{2}d(xxy)=xy\,dx+\frac{1}{2}xx\,dy$; so that now there is need

for no greater number of the fine theorems of celebrated men for Archimedean geometry, than at most those given by Euclid in his Book II or elsewhere, for ordinary geometry.

It was good to find that thereafter the calculus of transcendent quantities should reduce to ordinary quantities, and Huygens was especially pleased with this. Thus, if it is found that

$$2\int\frac{dy}{y} = 3\int\frac{dx}{x},$$

then from this we get $yy = x^3$, and this too from the nature of logarithms combined with the differential calculus, the former also being derived from the same calculus. For let $x^m = y$, then $mx^{n-1}\,dx = dy$. Hence, dividing each side by equal things, we have

$$m\frac{dx}{x} = \frac{dy}{y}.$$

Again, from the equation, $m \log x = \log y$, we have

$$\log x : \log y = \int\frac{dx}{x} : \int\frac{dy}{y}.$$ [113]

By this the exponential calculus is rendered practicable as well. For let $y^x = z$, then $x \log y = \log z$, $dx \log y + x\,dy : y = dz : z$.

In this way we free the exponents from the variable, or at other times we may transpose the variable exponent with advantage under the circumstances. Lastly, those things that were once held in high esteem are thus made a mere child's-play.

Now of all this calculus not the slightest trace existed in all the writings of his rival before the principles of the calculus were published by our friend;[114] nor indeed anything at all that Huygens or Barrow had not accomplished in the same way, in the cases where they dealt with the same problems.

But how great was the extent of the assistance afforded by the use of this calculus was candidly acknowledged by Huygens; and this his opponents suppress as much as ever they can, and straightway go on with other matters, not mentioning the real differential calculus in the whole of their report. Instead, they adhere to a large extent to infinite series, the method for which no one denies that his rival brought out in advance of all others. For those things which he said enigmatically, and explained at a much

later date, are all they talk about, namely, fluxions and fluents, i. e., finite quantities and their infinitely small elements; but as to how one can be derived from the other they offer not the slightest suggestion. Moreover, while he considers nascent or evanescent ratios, leading straight away from the differential calculus to the method of exhaustions, which is widely different from it (although it certainly also has its own uses), he proceeds not by means of the infinitely small, but by ordinary quantities, though these latter do finally become the former.

Since therefore his opponents, neither from the *Commercium Epistolicum* that they have published, nor from any other source, brought forward the slightest bit of evidence whereby it might be established that his rival used the differential calculus before it was published by our friend; therefore all the accusations that were brought against him by these persons may be treated with contempt as beside the question. They have used the dodge of the pettifogging advocate[115] to divert the attention of the judges from the matter on trial to other things, namely to infinite series. But even in these they could bring forward nothing that could impugn the honesty of our friend, for he plainly acknowledged the manner in which he had made progress in them; and in truth in these also, he finally attained to something higher and more general.

SUPPLEMENT.

Barrow, *Lectiones Geometricae,* Lect. XII, Prop. 1, 2, 3.

[Page 105, First Edition, 1670.]

General foreword. We will now proceed with the matter in hand; and, in order that we may save time and words, it is to be observed everywhere in what now follows that AB is some curved line, such as we shall draw, of which the axis is AD; to this axis all the straight lines BD, CA, MF, NG are applied perpendicular; the arc MN is indefinitely small; the straight line $\alpha\beta =$ arc AB, the straight line $\alpha\mu =$ arc AM, and $\mu\nu =$ arc MN; also lines applied to $\alpha\beta$ are perpendicular to it. On this understanding:

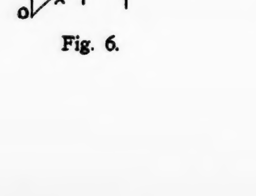


Fig. 6.

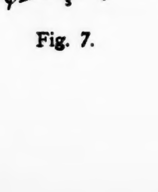


Fig. 7.

1. Let MP be perpendicular to the curve AB, and the lines KZL, $\alpha\phi\delta$ such that $FZ = MP$, $\mu\phi = MF$. Then the spaces $\alpha\beta\delta$, ADLK are equal.

For the triangles MRN, PFM *are similar,* $MN : NR = PM : MF$,

$$MN . MF = NR . PM;$$

that is, on substituting the equal quantities,

$$\mu\nu . \mu\phi = FG . FZ, \text{ or rect. } \mu\theta = \text{rect. } FH.$$

But the space $\alpha\beta\delta$ only differs in the slightest degree from an infinite number of rectangles such as $\mu\theta$, and the space ADLK is equivalent to an equal number of rectangles such as FH. Hence the proposition follows.

2. Hence, *if the curve* AMB *is rotated about the axis* AD, *the ratio of the surface produced to the space* ADLK *is that of the circumference of a circle to its diameter*; whence if the space ADLK is known, the said surface is known.

Some time ago I assigned the reason why this was so.

3. Hence, *the surfaces of the sphere, both the spheroids, and the conoids receive measurement.* For if AD is the axis of the conic section, etc.

NOTES.

[1] For abbreviations used in this article for these and other publications, see the list on pp. 483-485.

[2] G. 1848, p. 29; see also G. *math.*, III, pp. 71, 72, and Cantor, III, p. 40.

[3] A fair-minded consideration, like everything emanating from the pen of De Morgan, is given of the matter in a recent edition of his *Essays on the Life and Work of Newton.* The tale is told with the charm characteristic of De Morgan, and the edition is rendered very valuable by the addition of notes, commentary, and a large number of references supplied by the editor, P. E. B. Jourdain (Open Court Publishing Co.). Special attention is directed to De Morgan's summary of the unfairness of the case in Note 3 at the foot of pages 27-28.

[4] See under 11 below: also cf. the original Latin as given in G. 1846, p. 4, "*per amicum conscium.*"

[5] The account here given is substantially that given by Gerhardt in an article in Grunert's *Archiv der Mathematik und Physik,* 1856; pp. 125-132.

This article is written in contradiction to the view taken by Weissenborn in his *Principien der höheren Analysis,* Halle, 1856. It is worthy of remark that the partisanship of Gerhardt makes him omit in this article all mention of the review which Leibniz wrote for the *Acta Eruditorum* on Newton's work, *De Quadratura Curvarum,* which really drew upon him the renewal of the attack, by Keill. The passage which was objected to by the English mathematicians as being tantamount to a charge of plagiarism, in addition to the insult implied, according to their thinking, in making Newton the fourth proportional to Cavalieri, Fabri and Leibniz, is however given by Gerhardt in his preface to the *Historia* (G. 1846, p. vii). The following is a translation:

"Instead of the differences of Leibniz, Newton employs, and always has employed fluxions, which are as near as possible to augments of fluents—and these he has used both in his *Principia Naturæ Mathematica,* as well as in other works published later, with elegance; just as Honoratus Fabri in his *Synopsis Geometrica* has substituted increases of motions for the method of Cavalieri."

[6] Fatio's correspondence with Huygens is to be found in *Ch. Hugenii aliorumque seculi XVII virorum celebrium exercitationes mathematicae et philosophicae,* ed. Uylenbroeck, 1833.

[7] Bernoulli (Jakob), *Opera,* Vol. I, p. 431.

[8] *Ibid.,* p. 453.

[9] Cantor, III, p. 221.

[10] In the opening paragraph of the postscript, page 583.

[11] The account which follows is taken from Williamson's article, "Infinitesimal Calculus," in the *Times* edition of the *Encyc. Brit.* The memoir referred to contains a passage, of which the following is a translation (G., 1846, p. v):

"Perhaps the distinguished Leibniz may wish to know how I came to be acquainted with the calculus that I employ. I found out for myself its general principles and most of the rules in the year 1687, about April and the months following, and thereafter in other years; and at the time I thought that nobody besides myself employed that kind of calculus. Nor would I have known any the less of it, if Leibniz had not yet been born. And so let him be lauded by other disciples, for it is certain that I cannot do so. This will be all the more obvious, if ever the letters which have passed between the distinguished Huygens and myself come to be published. However, driven thereto by the very evidence of things, I am bound to acknowledge that Newton was the first, and by many years the first, inventor of this calculus; from whom, whether Leibniz, the second inventor, borrowed anything, I prefer that the decision should lie, not with me, but with others who have had sight of the paper of Newton, and other additions to this same manuscript. Nor does the silence of the more modest Newton, or the forward obtrusiveness of Leibniz...."

Truly another Roland in the field, and one in a vicious mood. What with other claimants to the method, such as Slusius, etc., at least as far as the differentiation of implicit functions of two variables is concerned, it would almost seem that the infinitesimal calculus, like Topsy, "just growed."

[12] See De Morgan's *Newton,* p. 26 and pp. 148, 149, where the Scholium is translated. The original Latin of this Scholium to Lemma II of Book II of the *Principia,* the altered Scholium that appeared in the second and third editions, with a note remarking on the change, will be found on pp. 48, 49, in Book II of the "Jesuits' Edition' 'of Newton (Editio Nova, edited by J. M. F. Wright, Glasgow, 1822; the third and best edition of the work by Le Sœur and Jacquier).

[13] *Phil. Trans.,* 1708; see also Cantor, III, p. 299.

[14] For a discussion, see Rosenburger, *Isaac Newton und seine physikalischen Principien,* Leipsic, 1895.

[15] The manner of the opening of this postscript would seem to indicate that something had been mentioned with regard to the matter of his irritation about imputed obligations to Barrow in the body of the letter; this cannot be ascertained, for Gerhardt does not quote the letter in connection.

[16] Leibniz can hardly with justice call Barrow his contemporary; Barrow anticipated him by half a dozen years at least. For Barrow had published his *Lectiones Geometricae* in 1670, while the very earliest date at which Leibniz could have obtained his results is the end of 1672; and there is reason to believe, as I have shown in my edition of the *Lectiones,* that Barrow was in possession of his method many years before publication, and had most probably communicated his secret to Newton in 1664.

[17] It is to be noted that the sole topic of this postscript is geometry, of which Leibniz candidly states that he knew practically nothing in 1672.

[18] Most probably the *Institutiones arithmeticae* of Johann Lantz, published at Munich in 1616; Cantor, III, p. 40.

[19] Possibly the *Geometria practica* of Christopher Clavius, better known as an editor of Euclid; he was the professor at Rome under whom Gregory St. Vincent studied. There are repeated references to Clavius in Cantor, II and III, Index, *q. v.*

It is worth remarking that neither Lanzius nor Clavius are mentioned in the *Historia.*

[20] It has been stated that, according to Descartes's own words, the intricacies of his *Geométrie* were intentional; it certainly has the character of a challenge to his contemporaries. There is no preparation, such as marks a book of the present day on coordinate geometry; Descartes starts straightway on the solution of a problem given up as insoluble by the ancients. No wonder that young Leibniz found some difficulty with his first attempt to read it.

[21] In 1635, Cavalieri published his *Geometria indivisibilibus,* and thus laid the foundation stone of the integral calculus. It would seem that Roberval was really the first inventor, or at least an independent inventor of the method; but he lost credit for it because he did not publish it, preferring to keep it to himself for his own use. Other examples of this habit are common among the mathematicians of the time.

[22] The book referred to was published in 1654. It appeared as the second volume of a work whose first volume was a critique and refutation of the quadrature of the circle published by Gregory St. Vincent; this second volume was not the work of Leotaud, as the second part of the title showed: "necnon CURVILINEORUM CONTEMPLATIO, olim inita ab ARTUSIO DE LIONNE, Vapincensi Episc." It therefore appears to have been an edited reprint of the work of De Lionne, the bishop of Gap (ancient name, Vapincum). Since part of this treatise is devoted to the "lunules of Hippocrates" (see Cantor, I, pp. 192-194), it may have had some influence with Leibniz in giving him the first idea for his evaluation of π.

[23] Literally, "I was about to swim without corks."

[24] Leibniz here would appear to assert that he had considered some form of rectangular coordinate geometry, the association with the name of Descartes being fairly conclusive. Vieta's *In Artem Analyticam Isagoge,* explained how algebra could be applied to the solution of geometrical problems (Rouse Ball); for further information see Cantor.

[25] This seems to have been an improvement on the adding machine of Pascal, adapting it to multiplication, division and extraction of roots. Pascal's machine was produced in 1642, and Leibniz's in 1671.

[26] Huygens's *Horologium Oscillatorium* was published in 1673; we are thus provided with an exact date for the occurrence of the conversation that set Leibniz on to read Pascal and St. Vincent. This was after his first visit to London, from which he returned in March, "having utilized his stay in London to purchase a copy of Barrow's *Lectiones,* which Oldenburg had brought to his notice" (Zeuthen, *Geschichte der Mathematik im XVI. und XVII. Jahrhundert*; German edition by Mayer, p. 66). Leibniz himself mentions in a letter to Oldenburg, dated April 1673, that he has done so. Gerhardt (G. 1855, p. 48) states that he has seen, in the Royal Library of Hanover the copy of Barrow's *Lectiones Geometricae,* so that it must have been the combined edition of the Optics and the Geometry, published in 1670, that Leibniz bought.

Thus, before he is advised to study Pascal by Huygens, he has already in his possession a copy of Barrow. It is idle that any one should suppose that Leibniz bought this book on the recommendation of a friend in order merely to possess it; Leibniz bought books, or borrowed them, for the sole purpose of study. Unless we are to look upon this account of his reading as the result of lack of memory extending back for thirty years, there is only one conclusion to come to, barring of course the obviously brutal one that Leibniz lied; and this conclusion is that at the first reading the only thing that Leibniz could follow in Barrow was the part that he marked *Novi dudum* ("Knew this before"), and this was the appendix to Lecture XI, which dealt with the *Cyclometria* of Huygens, as Barrow calls the book entitled *De Circuli Magnitudine Inventa.* The absence of any more such remarks is almost proof positive that Leibniz knew none of the rest before. Hence he must have read the Barrow before he had filled those "hundreds of sheets" that he speaks of

later, with geometrical theorems that he has discovered; for at the end of the postscript we are considering he states that "in Barrow, *when his Lectures appeared,* I found the greater part of my theorems anticipated." There is something very wrong somewhere; for this would appear to state that it was the second edition of Barrow, published in 1874, that Leibniz had bought; it is impossible, as the words of Leibniz stand, that they should refer to the 1670 edition, for it had been published before Leibniz arrived in Paris. It is however certain from Leibniz's letter to Oldenburg that it could not be the 1674 edition, for the date of the letter is 1673.

In this letter Leibniz only makes a remark on the optical portion; but it could not have been the separate edition of the Optics, published in 1669, for Gerhardt states that the copy he has seen contains the Geometry with notes in the margin.

To those who have ever waded through the combined edition of Barrow's Optics and Geometry, it may be that rather a startling suggestion will occur. It was sheer ill-luck that drove Leibniz, after studying the Optics (perhaps on the journey back from London, for we know that this was a habit of his), to get tired of the five preliminary geometrical lectures in all their dryness, and on reaching home, just to skim over the really important chapters, *missing all the important points,* and just the name of Huygens catching his eye. This is a new suggestion as far as I am aware; everybody seems to decide between one of two things, either that Leibniz never read the book until the date he himself gives, "*Anno Domini* 1675 as far as I remember," or else that he purposely lied. I will return to this point later; meanwhile see Cantor, III, pp. 161-163, and consult the references given in the footnotes to these pages; the pros and cons of the conflict between probability and Leibniz's word are there summarized.

[27] Pascal's chief work on centers of gravity is in connection with the cycloid, and solids of revolution formed from it. His method was founded on the indivisibles of Cavalieri. His work was issued as a challenge to contemporaries under the assumed name of Amos Dettonville, and under the same name he published his own solutions, after solutions had been given by Huygens, Wallis, Wren and others.

[28] The method of *ductus plani in planum,* the leading or multiplication of a plane into a plane, employed by Gregory St. Vincent in the seventh book of his *Opus Geometricum* (1649) is practically on the same fundamental principle as the present method of finding the volume of a solid by integration. A simple explanation may be given by means of the figure of a quarter of a cone. Let AOBC be the quarter of a circular cone as Fig. A of which OA is the axis, and ABC the base, so that all sections, such as *abc,* are parallel to ABC and perpendicular to the plane AOC. Let *ad* be the height of a rectangle equal in area to the quadrant *abc,* so that *ad* is the average height of the

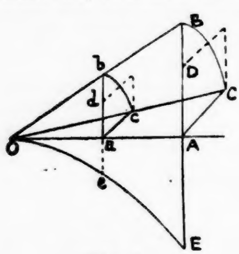


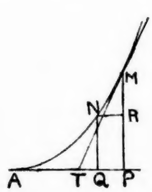
Fig. A.

variable plane *abc*; then the volume of the figure is found by multiplying the height of the variable plane as it moves from O to the position ABC by the corresponding breadth of the plane OAC, i. e., by *bc,* and adding the results.

As we shall see later, Leibniz does not fully appreciate the real meaning of the method; on the other hand Wallis uses the method with good effect in his *Arithmetica Infinitorum*, and states that he has come to it independently. In the above case he would have stated that the product in each case was proportional to the square on *ac*, drawn an ordinate *ae* at right angles to O*a*, so that *ae* represented the product, and so formed the parabola O*e*EA*a*O, of which the area is known to him. This area is proportional to the volume of the cone.

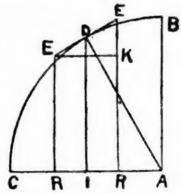
[29] *Ungulae* denote hoof-shaped solids, such as the frusta of cylinders or cones cut off by planes that are not parallel to one another.

[30] The figure here given is of extreme interest. First of all it is not Barrow's "differential triangle," which is that of Fig. B below; this of course is only what those who believe Leibniz's statement that he received no help from Barrow, would expect. By the way, the figure given by Cantor as Barrow's is not quite accurate. (Cantor, III, p. 135.)



BARROW

Fig. B.



PASCAL

Fig. C.

But neither is it the figure of Pascal, which is that of Fig. C. Of course, I am assuming that Gerhardt has given a correct copy of the figure given by Leibniz in his manuscript; although that which I have given of it, a faithful copy of Gerhardt's, shows that his curve was not a circle. I also assume that Cantor is correct in the figure that he gives from Pascal; although Cantor says that the figure occurs in a tract on the sines of a quadrant, and not, as Leibniz states, in a problem on *the measurement of the sphere*. Indeed it seems to me that the figure is more likely to be connected with the area of the zone of a sphere and the proof that this is equal to the corresponding belt on the circumscribing cylinder than anything else. I am bound to assume these things, for I have not had the opportunity of seeing either of the figures in the original for myself. It is strange, in this connection, that Gerhardt in one place (G. 1848, p. 15) gives 1674 as the date of the publication of Barrow, and in another place (G. 1855, p. 45) seven years later, he makes it 1672, and neither of them are correct as the date of the copy that Leibniz could possibly have purchased, namely 1670. This is culpable negligence in the case of a date upon which an argument has to be founded, for one can hardly suspect Gerhardt of deliberate intent to confuse. Nevertheless, like De Morgan, I should have felt more happy if I could have given facsimiles of Barrow's book, and Leibniz's manuscript and figure.

Lastly, there is in Barrow (what neither Gerhardt, Cantor, nor any one else, with the possible exception of Weissenborn, seem to have noticed) chapter and verse for Leibniz's "characteristic triangle." Fig. D is the diagram that Barrow gives to illustrate the first theorem of Lecture XI. This is of course, as is usual with Barrow, a complicated diagram drawn to do duty for a whole set of allied theorems.

In the proof of the first of these theorems occur these words:

"Then the triangle HLG is similar to the triangle PDH (for, on account of the infinite section, the small arc HG can be considered as a straight line).

Hence, HL : LG = PD : DH, or HL . DH = LG . PD,
i. e., HL . HO = DC . Dψ.

By similar reasoning, it may be shown that, since the triangle GMF is similar to the triangle PCG,...."

If now the lines in italics are compared with that part of the figure to which they refer, which has been abstracted in Fig. E, the likeness to Leib-

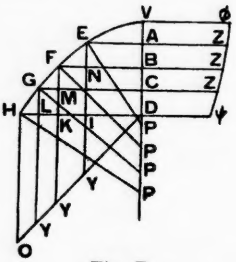


Fig. D.

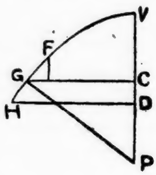


Fig. E.

niz's figure wants some explaining away, if we consider that Leibniz had the opportunity for seeing this diagram. Such evidence as that would be enough to hang a man, even in an English criminal court. (Further, see note 46.)

To sum up, I am convinced that Leibniz was indebted to both of Barrow's diagrams, and also to that of Pascal (for I will call attention to the fact that he uses all three, as I come to them) and I think that after the lapse of thirty years he really could not tell from whom he got his figure. In such a case it would be only natural, if he knew that it was from one of two sources and he was accused of plagiarizing from the one, that he should assert that it was from the other. Hence, by repetition, he would come to believe it. But even this does not explain his letter to d'Hôpital, where he says that he has not obtained any assistance from his methods; unless again we remember that this letter is dated 1694, twenty years after the event.

[31] Great importance, in my opinion hardly merited, is attached to the use by Leibniz of the phrase *momento ex axe* in this place, and in his manuscripts under the heading *Analysis Tetragonistica ex Centrobarycis,* dated October, 1675.

The Latin word *momentum,* a contraction of *movimentum,* has a primary meaning of movement or alteration, and a secondary meaning of a cause producing such movement. The present use of the term to denote the tendency of a force to produce rotation is an example of the use of the word to denote an effect; from the second idea, we have first of all its interpretation as something just sufficient to cause the alteration in the swing of a balance (where the primary idea still obtains), hence something very small, and especially a very small element of time.

Thus we see that Leibniz uses the term in its primary sense, for he employs it in connection with a method *ex Centrobarycis,* and in its mechanical sense, and it is thus fairly justifiable to assume that he got the term from Huygens; in just this sense we now speak of the moment of inertia.

Newton's use of the term is given in Lemma II of Book II of the *Principia,* in the following way.

"I shall here consider such quantities as undetermined or variable, as it were increasing or decreasing by a continual motion or flow (*fluxus*); and their instantaneous (*momentanea*) increments or decrements I shall denote (*intelligo* = understand) by the name "moments"; so that increments stand for moments that are added or positive (*affirmativis*), and decrements for those that are subtracted or negative."

This has nothing whatever to do with what Leibniz means by a moment,

and it seem ridiculous to bring forward the use of this word as evidence that Leibniz had seen Newton's work, or even heard of it through Tschirnhaus, before the year 1675.

The fact that in another place, where I will refer to it again, he uses the phrase "instantaneous increment" is quite another matter.

The use of the word moment in this mechanical sense is here perfectly natural. See Cantor, III, p. 165; also Cantor, II, p. 569, where the *idea* is referred back at least to Benedetti (1530-1590); but the idea is fundamental in the theorems due to Pappus concerning the connection between the path of the center of gravity of an area and the surfaces and volumes of rings generated by the area, of which the proofs were given by Cavalieri. When, however, and by whom, the *word* moment was itself first used in this connection, I have been unable to find the slightest trace.

[32] With due regard to the statement that Leibniz "had looked through Cavalieri" before he went to Paris, it is not remarkable that he did not notice very much at all in Cavalieri. Cavalieri's *Geometria indivisibilibus* is not a book to be "looked through." It is a work for weeks of study. I cannot say whether the idea involved in Leibniz's characteristic triangle is used by Cavalieri as such; but I do not see how else he could have given proofs (as stated by Williamson in his article on "Infinitesimal Calculus" in the *Times* edition of the *Encyc. Brit.*) of Pappus's theorem for the area of a ring; and I should think that it is morally certain that Cavalieri is the source from which Wallis obtained his ideas for the rectification of the arc of the spiral. I had occasion to refer to a copy in the Cambridge University Library, and what I saw of it in the short time at my disposal determined me to make a translation of it, with a commentary, *as soon as I had enough time at my disposal.* "As one reads tales of romance"!

[33] Note that this is proportional to the area of the surface formed by the revolution of the curve C(C) about the axis AP. Barrow does not use the method to find the areas of surfaces of revolution; he prefers to straighten out the curve C(C), and erect the ordinates BC, (B)(C) perpendicular to the curve thus straightened; i. e., he works with the product BC.C(C) as it stands. But, after giving the determination of the surface of a right circular cone as an example of the method, and as a means of combating the objections of Tacquet to the method of indivisibles, he goes on to say: "Evidently in the same manner we can investigate most easily the surfaces of spheres and portions of spheres (nay, provided all necessary things are given or known, any other surfaces that are produced in this way). But I propose to keep, to a great extent, to more general methods" (end of Lecture II). Thus we find that Barrow does not give any further examples of the determination of the areas of surfaces of revolution until Lecture XII. And why? Because he is not writing a work on mensuration, but a calculus. The reference to the method of indivisibles however shows that in Barrow's opinion, if Cavalieri had not used his method for the determination of the area of the surface of a sphere, then he ought to have done so.

[34] It is difficult to see also how Huygens could have performed his constructions unless he had used the method that Leibniz claims to have discovered.

[35] It is strange that Roberval, as an independent discoverer of the method of indivisibles, did not perceive the method of the constructions of Huygens. Of Bullialdus I can find no mention except as the author of a set of navigation tables. Cantor does not even refer to him, as far as I can find.

[36] This conversation probably took place late in 1673; see a note on the alteration of the date of a manuscript dated November 11, 1673, where the 3 was originally a 5 (Section below).

The method of Slusius (de Sluze, or Sluse) is as follows:

Suppose that the equation of the given curve is

$$x^3 - 2x^2y + bx^2 - b^2x + by^2 - y^3 = 0.$$

Slusius takes all the terms containing y, multiplies each by the corresponding index of y; then similarly takes all the terms containing x, multiplies each by the corresponding index of x, and divides each term of the result by x; the quotient of the former by the last expression gives the value of the subtangent. This is practically the content of Newton's method of *analysis per aequationes,* and Slusius sent an account of it to the Royal Society in January, 1673. It was printed in the *Phil. Trans.,* as No. 90. This is given by Gerhardt (G. 1848, p. 15) as an example of the method of Slusius. It is rather peculiar that Gerhardt does not mention that this is the example given by Newton in the oft-quoted letter of December 10, 1672, and represents what Newton "guesses the method to be." As it stands in G. 1848, it would appear to be a quotation from the work of Slusius himself. There is evidence that Leibniz had seen the explanation given in the *Phil. Trans.,* or had been in communication with Slusius; this will be referred to later, but it may be said here that this fact makes Leibniz somewhat independent of any necessity of having seen Newton's letter.

[37] Some point is made of the question why, if Leibniz had seen the "differential triangle" of Barrow, he should have called it by a different name. If there were any point in it at all, it would go to prove that Barrow's calculus was published by Barrow as a *differential* calculus. But there is no point, for Barrow never uses the term! It is a product of later growth, by whom first applied I know not. Leibniz, thus free to follow his logical plan of denominating everything, uses a term borrowed from his other work. He thus defines a character or characteristic. "Characteristics are certain things by means of which the mutual relations of other things can be expressed, the latter being dealt with more easily than are the former." See Cantor, III, p. 33f.

[38] Gregory's *Geometriae Pars Universalis* was published at Padua in 1668. Leibniz had either this book, or the Barrow in which one of Gregory's theorems is quoted, close at hand in his work. For he gives it as an example of the power of his calculus, referring to a diagram which is not drawn. This diagram I was unable to draw from the meager description of it given by Leibniz, until *I looked up Barrow's figure,* in default of being able to obtain a copy of Gregory's work; thereupon the figure was drawn immediately.

[39] Here indeed it must be admitted that Leibniz is —suffering from a lapse of memory. As has been said before, Barrow's lectures appeared in 1670 and were in the possession of Leibniz before ever he dreamed of his theorems. But what can one expect when admittedly this account (from which the *Historia* was in all probability written up) is purely from memory, aided by the few manuscripts that he had kept. Gerhardt does not say that he has found, nor does he publish, any manuscripts that could possibly give the order in which the text-books that Leibniz procured were read. Which of us, at the age of 57, could say in what order we had read books at the age of 27; or, if by then we had worked out a theory, could with accuracy describe the steps by which we climbed, or from a mass of muddle and inaccuracies, say to whom we were indebted for the first elementary ideas that we had improved beyond all recognition? I doubt whether any of us would recognize our own work under such circumstances.

[40] Again Leibniz makes a bad mistake in affecting to despise the work of his rivals—for that is what the words, "these things were perfectly easy to the veriest beginner who had been trained to use them," makes us believe. It is also bad taste, for, besides Barrow, Huygens also remained true to the method of geometry till his death. The sentence which follows is "pure swank," and as a matter of fact it was left to others, such as the Bernoullis, to make the best use of the method of Leibniz. The great thing we have to thank Leibniz for is the notation; it is a mistake to call this the invention of a notation for the infinitesimal calculus. As we shall see, Leibniz invented this notation for finite differences, and only applied it to the case in which the differences were infinitely small. Barrow's method, of a and e, also survives to the present day, under the disguise of h and k, in the method by which the

elements of the calculus are taught in nine cases out of ten. For higher differential coefficients the suffix notation is preferable, and later on the operator D is the method *par excellence.*

[41] Here Leibniz seems to be unable to keep from harking back to the charge made by Fatio, suggesting that by the publication of his letters by Wallis this charge has been proved to be absolutely groundless.

[42] It is probable that this may mean "has received high commendation"; for *elogiis* may be the equivalent of eulogy, in which case *celebratus est* must be translated as "has been renowned."

[43] This is untrue. As has been said, the attack was first made publicly in 1699; at this time, although Huygens had indeed been dead for four years, Tschirnhaus was still alive, and Wallis was appealed to by Leibniz. It is strange that Leibniz did not also appeal to Tschirnhaus, through whom it is suggested by Weissenborn that Leibniz may have had information of Newton's discoveries. Perhaps this is the reason why he did not do so, since Tschirnhaus might not have turned out to be a suitable witness for the defense. Leibniz must have had this attack by Fatio in his mind, for he could hardly have referred to Keill as a *novus homo,* while we know that he did not think much of Fatio as a mathematician. To say that there never existed any uncertainty as to the name of the true inventor until 1712 is therefore sheer nonsense; for if by that he means to dismiss with contempt the attack of Fatio, who can he mean by the phrase *novus homo?* The sneering allusion to "the hope of gaining notoriety by the discussion" can hardly allude to any one but Fatio. Finally if Fatio is dismissed as contemptible, the second attack by Keill was made in 1708. If it was early in the year, Tschirnhaus was even then alive, though Wallis was dead.

[44] Gerhardt says in a note (G. 1846, p. 22) that his real name was probably Kramer; for what reason I am unable to gather. Cantor says distinctly that his name was Kaufmann, and this is the usually accepted name of the man who was one of the first members of the Royal Society and contributed to its *Transactions.* It seems to me that Gerhardt is guessing; the German word *Kramer* means a small shopkeeper, while *Kaufmann* means a merchant. To Mercator is due the logarithmic series obtained by dividing unity by $(1+x)$ and integrating the resulting series term by term; the connection with the logarithm of $(1+x)$ is through the area of the rectangular hyperbola $y(1+x)=0$. See Reiff, *Geschichte der unendlichen Reihen.*

[45] Newton obtained the general form of the binomial expansion after the method of Wallis, i. e., by interpolation. See Reiff.

[46] We now see what was Leibniz's point; the differential calculus was not the employment of an infinitesimal and a summation of such quantities; it was the use of the idea of these infinitesimals being differences, and the employment of the notation invented by himself, the rules that governed the notation, and the fact that differentiation was the inverse of a summation; and perhaps the greatest point of all was that the work had not to be referred to a diagram. This is on an inestimably higher plane than the mere differentiation of an algebraic expression whose terms are simple powers and roots of the independent variable.

[47] Why is Barrow omitted from this list? As I have suggested in the case of Barrow's omission of all mention of Fermat, was Leibniz afraid to awake afresh the sleeping suggestion as to his indebtedness to Barrow? I have suggested that Leibniz read his Barrow on his journey back from London, and perhaps, tiring at having read the Optics first and then the preliminary five lectures, just glanced at the remainder and missed the main important theorems. I also make another suggestion, namely, that perhaps, or probably, in his then ignorance of geometry he did not understand Barrow. If this is the case it would have been gall and wormwood for Leibniz to have ever owned to it. Then let us suppose that in 1674 with a fairly competent knowl-

edge of higher geometry he reads Barrow again, skipping the Optics of which he had already formed a good opinion, and the wearisome preliminary lectures of which he had already seen more than enough. He notes the theorems as those he has himself already obtained, and the few that are strange to him he translates into his own symbolism. I suggest that this is a feasible supposition, which would account for the marks that Gerhardt states are made in the margin. It would account for the words "in which latter I found the greater part of my theorems anticipated" (this occasion in future times ranking as the first time that he had really read Barrow, and lapse of memory at the end of thirty years making him forget the date of purchase, possibly confusing his two journeys to London); it would account for his using Barrow's differential triangle instead of his own "characteristic triangle." As Barrow tells his readers in his preface that "what these lectures bring forth, or to what they may lead you may easily learn from *the beginnings of each*," let us suppose that Leibniz took his advice. What do we find? The first four theorems of Lecture VIII give the geometrical equivalent of the differentiation of a power of *a dependent* variable; the first five of Lecture IX lead to a proof that, expressed in the differential notation,

$$(ds/dx)^2 = 1 + (dy/dx)^2;$$

the appendix to this lecture contains the differential triangle, and five examples on the *a* and *e* method, fully worked out; the first theorem in Lecture XI has a diagram such that, when that part of it is dissected out (and Barrow's diagrams want this in most cases) which applies to a particular paragraph in the proof of the theorem, this portion of the figure is a *mirror image of the figure* drawn by Leibniz when describing the characteristic triangle (turn back to note 30). I shall have occasion to refer to this diagram again. The appendix to this lecture opens with the reference to the work of Huygens; and the second theorem of Lecture XII is the strangest coincidence of all. This theorem in Barrow's words is:

"Hence, if the curve AMB is rotated about the axis AD, the ratio of the surface produced to the space ADLK is that of the circumference of a circle to its diameter; whence, if the space ADLK is known, the said surface is known."

The diagram given by Barrow is as usual very complicated, serving for a group of nine propositions. Fig. F is that part of the figure which refers to the theorem given above, dissected out from Barrow's figure. Now remember that Leibniz always as far as possible kept his axis clear on the left-hand side of his diagram, while Barrow put his datum figure on the left of his axis, and his constructed figures on the right; then you have Leibniz's diagram and the proof is by the similarity of the triangles MNR, PMF, where

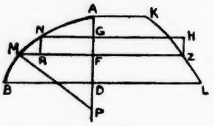


Fig. F.

FZ = PM; and the theorem itself is only another way of enunciating the theorem that Leibniz states he generalized from Pascal's particular case! Lastly, the next theorem starts with the words: "Hence the surfaces of the sphere, both the spheroids and the conoids receive measurement." What a coincidence!

As this note is getting rather long, I have given the full proof of the first two theorems of Barrow's Lecture XII as an appendix, at the end of this section.

The sixth theorem of this lecture is the theorem of Gregory which Leibniz also gives later; I will speak of this when I come to it. As also, when we

discuss Leibniz's proof of the rules for a product, etc., I will point out where they are to be found in Barrow ready to his hand.

Yet if all this were so, he could still say with perfect truth that, in the matter of the invention of the differential calculus (as he conceived the matter to consist, that is, the differential and integral notations and the method of analysis), he derived no assistance from Barrow. In fact, once he had absorbed his fundamental ideas, Barrow would be less of a help than a hindrance.

[48] Apollonian geometry comprised the conic sections or curves of the second degree according to Cartesian geometry; curves of a higher degree and of a transcendent nature, like the spiral of Archimedes, were included under the term "mechanical."

[49] The great discovery of Descartes was not simply the application of geometry; that had been done in simple cases ages before. Descartes recognized the principle that every property of the curve was included in its equation, if only it could be brought out. Thus Leibniz's greatest achievement was the recognition that the differential coefficients were also functions of the abscissa. The word functon was applied to certain straight lines dependent on the curve, such as the abscissa itself, the ordinate, the chord, the tangent, the perpendicular, and a number of others (Cantor, III, preface, p. v). This definition is from a letter to Huygens in 1694. There is therefore a great advance made by 1714, the date of the *Historia,* since here it is at least strongly hinted that Leibniz has the algebraical idea of a function.

[50] With regard to Newton, at least, this is untrue. Without a direct reference to the original manuscript of Newton it is quite impossible to state whether even Newton *wrote* 0 or o; even then there may be a difficulty in deciding, for Gerhardt and Weissenborn have an argument over the matter, while Reiff prints it as 0. However this may be there is no doubt that Newton considered it as an infinitely small *unit of time,* only to be put equal to zero when it occurred as a factor of terms in an expression in which there also occurred terms that did not contain an infinitesimally small factor. This was bound to be the case, since Newton's $\dot{x}$ and $\dot{y}$ were velocities. In short, expressing Newton's notation in that of Leibniz, we have

$$\dot{x}o \text{ or } \dot{x}0 = (dx/dt).\ dt$$

and therefore $\dot{x}o$ is an infinitesimal or a differential equal to Leibniz's dx.

[51] This is in a restricted sense true. No one seems to have felt the need of a second differentiation of an original function; those who did differentiated once, and then worked upon the function thus obtained a second time in the same manner as in the first case. Barrow indeed only considered curves of continuous curvature, and the tangents to these curves; but Newton has the notation $\ddot{x}$, etc. But the idea had been used by Slusius in his *Mesolabum* (1659), where a general method of determining points of inflection is made to depend on finding the maximum and minimum values of the subtangent. Lastly, it can hardly be said that Leibniz's interpretation of $\int\int$ ever attained to the dignity of a double integral in his hands.

[52] David Gregory is not the only sinner! Leibniz, using his calculus, makes a blunder over osculations, and will not stand being told about it; he simply repeats in answer that he is right (Rouse Ball's *Short History*).

[53] The names of the committee were not even published with their report. In fact the complete list was not made public until De Morgan investigated the matter in 1852! For their names see De Morgan's *Newton,* p. 27.

[54] What then made Leibniz change his mind?

[55] It is established that this was Johann (John) Bernoulli; see Cantor, III, p. 313f; Gerhardt gives a reference to Bossut's *Geschichte,* Part II, p. 219.

[56] This seems to be an intentional misquotation from Bernoulli's letter, which stated that Newton did not understand the meaning of higher differen-

tiations. At least, that is what Cantor says was given in the pamphlet; and it is established that....

[57] the pamphlet referred to was also an anonymous contribution by Leibniz himself! Is it strange that hard things are both thought and said of such a man?

[58] Again this is Leibniz himself! Had he then no friends at all to speak for him and dare subscribe their signatures to the opinion? Unfortunately Tschirnhaus was dead at the time of the publication of the *Commercium Epistolicum,* but he could have spoken with overwhelming authority, as Leibniz's co-worker in Paris, at any time between the date of Leibniz's review of Newton's *De Quadratura* in the *Acta Eruditorum* until his death in 1708, even if he had died before the publication of Keill's attack in the *Phil. Trans.* of that year was made known to him. Does not this silence on the part of Tschirnhaus, the personal friend of Leibniz, rather tend to make Leibniz's plea, that his opponents had had the shrewdness to wait till Tschirnhaus, among others, was dead, recoil on his own head, in that he has done the very same thing? Leibniz must have known the feeling that this review aroused in England, and, Huygens being dead, Tschirnhaus was his only reliable witness. Of course I am not arguing that Leibniz did found his calculus on that of Newton. I am fully convinced that they both were indebted to Barrow, Newton being so even more than Leibniz, and that they were perfectly independent of one another in the development of the *analytical* calculus. Newton, with his great knowledge of and inclination toward geometrical reasoning, backed with his personal intercourse with Barrow, could appreciate the finality of Barrow's proofs of the differentiation of a product, quotient, power, root, logarithm and exponential, and the trigonometrical functions, in a way that Leibniz could not. But Newton never seems to have been accused of plagiarism from Barrow; even if he had been so accused, he probably had ready as an answer, that Barrow had given him permission to make any use he liked of the instruction that he obtained from him. Leibniz, when so accused, replied by asserting, through confusion of memory I suggest, that he got his first idea from the works of Pascal. Each developed the germ so obtained in his own peculiar way; Newton only so far as he required it for what he considered his main work, using a notation that was of greatest convenience to him, and finally falling back on geometry to provide himself with what appealed to him as rigorous proof; Leibniz, more fortunate in his philosophical training and his lifelong effort after symbolism, has ready to hand a notation, almost developed and perfected when applied to finite quantities, which he saw with the eye of genius could be employed as usefully for infinitesimals. De Morgan justly remarks that one dare not accuse either of these great men of deliberate untruth with regard to specific facts; but it must be admitted that neither of them can be considered as perfectly straightforward; and the political similitude, which Cantor speaks of, in which nothing is too bad to be said of an opponent, seems to have applied just as much to the mathematician of the day as to the politician.

[59] This was given in more detail in the first draught of this essay (G. 1846, p. 26): Hitherto, while still a pupil, he kept trying to reduce logic itself to the same state of certainty as arithmetic. He perceived that occasionally from the first figure there could be derived a second and even a third, without employing conversions (which themselves seemed to him to be in need of demonstration), but by the sole use of the principle of contradiction. Moreover, these very conversions could be proved by the help of the second and third figures, by employing theorems of identity; and then now that the conversion had been proved, it was possible to prove a fourth figure also by its help, and this latter was thus more indirect than the former figures. He marveled very much at the power of identical truths, for they were generally considered to be useless and nugatory. But later he considered that the whole of arithmetic and geometry arose from identical truths, and in general that all undemonstrable truths depending on reasoning were identical, and that these combined

with definitions yield identical truths. He gave as an elegant example of this analysis a proof of the theorem, The whole is greater than its part.

[60] It is fairly certain that Leibniz could not possibly at this time have perceived that in this theorem he has the germ of an integral. The path to the higher calculus lay through geometry. As soon as Leibniz attained to a sufficient knowledge of this subject he would recognize the area under a curve between a fixed ordinate and a variable one as a set of magnitudes of the kind considered, the ordinates themselves being the differences of the set; he would see that there was no restriction on the number of steps by which the area attained its final size. Hence, in this theorem he has a proof to hand that integration as a determination of an area is the inverse of a difference. This does not mean the inverse of a differentiation, i. e., the determination of a *rate,* or the drawing of a tangent. As far as I can see, Leibniz was far behind Newton in this, since Newton's fluxions were founded on the idea of a rate.

[61] It is a pity that we are not told the date at which Leibniz read his Wallis; it is a greater pity that Gerhardt did not look for a Wallis in the Hanover Library and see whether it had the date of purchase on it (for I have handled lately several of the books of this time, and in nearly every case I found inserted on the title page the name of the purchaser and the date of purchase). I make this remark, because there arises a rather interesting point. Wallis, in his *Arithmetica Infinitorum,* takes as the first term of all his series the number 0, and in one case he mentions that the differences of the differences of the cubes is an arithmetical series. He also works out fully the sums of the figurate numbers (or as Leibniz calls them the combinatory numbers), the general formulas for these sums he calls their *characteristics.* He also remarks on the fact that any number can be obtained by the addition of the one before it and the one above it (which is itself the sum of all the numbers in the preceding column above the one to the left of that which he wishes to obtain). Thus, in the fourth column 4 is the sum of 3 (to the left) and I (above), i. e., the sum of the two first numbers in column three; 10 is the sum of 6 (to the left) and 4 (above, which has been shown to be the sum of the first two numbers of column three), and therefore 10 is the sum of the first three numbers in column three. Now my point is, assuming it to have been impossible that Leibniz had read Wallis at the time that he was compiling his *De Arte,* we have here another example, free from all suspicion, of that series of instances of independent contemporary discoveries that seems to have dogged Leibniz's career.

[62] The name surdesolid to denote the fifth power is used by Oughtred, according to Wallis. By Cantor the invention of the term seems to be credited to Dechales, who says, "The fifth number from unity is called by some people the quadrato-cubus, but this is ill-done, since it is neither a square nor a cube and cannot thus be called the square of a cube nor the cube of a square: we shall call it supersolidus or surde solidus." (Cantor, III, p. 16.)

[63] This theorem is one of the fundamental theorems in the theory of the summation of series by finite differences, namely,

$$\Delta^m u_n = n_{n+m} - {}_mC_1 \cdot u_{n+m-1} + {}_mC_2 \cdot u_{n+m-2} - \text{etc.},$$

which is usually called the direct fundamental theorem; for although Leibniz could not have expressed his results in this form since he did not know the sums of the figurate numbers as generalized formulas (or I suppose not, if he had not read Wallis), and apparently his is only a general case, yet it must be remembered that any term of the first series can be chosen as the first term. It is interesting to note that the second fundamental theorem, the inverse fundamental theorem, was given by Newton in the *Principia,* Book III, lemma V, as a preliminary to the discussion on comets at the end of this book. Here he states the result, without proof, as an interpolation formula; (it is frequently referred to as Newton's Interpolation Formula); it may however be used as an extrapolation formula, in which case we have

$$u_{m+n} = u_m + {}_nC_1 \cdot \Delta u_m + {}_nC_2 \cdot \Delta^2 u_m + \text{etc.}$$

In the two formulas as given here, the series are

$$\begin{array}{lllllll} u_1 & u_2 & u_3 & u_4 & u_5 & \text{etc.} & \\ & \Delta u_1 & \Delta u_2 & \Delta u_3 & \Delta u_4 & \text{etc.} & \\ & & \Delta^2 u_1 & \Delta^2 u_2 & \Delta^2 u_3 & \text{etc.} & \text{and so on.} \end{array}$$

[64] What are we to understand by the inclusion of this series in this connection? Does Leibniz intend to claim this as his? I have always understood that this is due to John Bernoulli, who gave it in the *Acta Eruditorum* for 1694, in a slightly different form, and proved by direct differentiation; and that Brook Taylor obtained it as a particular case of a general theorem in and by *finite differences.* If Leibniz intended to claim it, he has clearly anticipated Taylor. It is quite possible that Leibniz had done so, even in his early days; and as soon as in 1675, or thereabouts, he had got his signs for differentiation and integration, it is possible that he returned to this result and expressed it in the new notation; for the theorem follows so perfectly naturally from the last expression given for $a - \omega$. But it is hardly probable, for Leibniz would almost certainly have shown it to Huygens and mentioned it.

The other alternative is that here he is showing how easily Bernoulli's series could have been found in a much more *general form,* i. e., as a theorem that is true (as he indeed states) for finite differences as well as for infinitesimals; the inclusion of this statement makes it very probable that this supposition is a correct one. This leads to a pertinent, or impertinent, question. Brook Taylor's *Methodus Incrementorum* was published in 1715; the *Historia* was written some time between 1714 and 1716; Gerhardt states that there were two draughts of the latter, and that he is giving the second of these. In justice to Leibniz there should be made a fresh examination of the two draughts, for if this theorem is not given in the original draught it lays Leibniz open to further charge of plagiarism. I fully believe that the theorem will be found in the first draught as well and that my alternative suggestion is the correct one.

In any case, the tale of the *Historia* is confused by the interpolation of the symbolism invented later (as Leibniz is careful to point out). The question is whether this was not intentional. And this query is not impertinent, considering the manner in which Leibniz refrains from giving dates, or when we compare the essay in the *Acta Eruditorum,* in which he gives to the world the description of his method. Weissenborn considers that "this is not adapted to give an insight into his methods, and it certainly looks as if Leibniz wished deliberately to prevent this." Cf. Newton's "anagram" (sic), and the Geometry of Descartes, for parallels.

[65] In reference to the employment of the calculus to diagrammatic geometry, as will be seen later, Leibniz says:

"But our young friend quickly observed that the differential calculus could be employed with figures in an even more wonderfully simple manner than it was with numbers, because *with figures the differences were not comparable with the things which differed*; and as often as they were connected together by addition or subtraction, being incomparable with one another, the less vanished in comparison with the greater."

[66] This makes what has just gone before date from the time previous to his coming across Cavalieri. See note following.

[67] This is about the first place in which it is possible to deduce an exact date, or one more or less exact. According to Leibniz's words that immediately follow it may be deduced that it was somewhere about twelve months before the publication of the Hypothesis of Physics—if we allow for a slight interval between the dropping of the geometry and the consideration of the principles of physics and mechanics, and a somewhat longer interval in which to get together the ideas and materials for his essay—that he had finished his

"slight consideration" of Leotaud and Cavalieri. This would make the date 1670, and his age 24.

[68] This essay founded the explanation of all natural phenomena on motion, which in turn was to be explained by the presence of an all-pervading ether; this ether constituted light.

[69] The dedication of the *Nova methodus* in 1667 to the Elector of Mainz (ancient name Moguntiacum) procured for Leibniz his appointment in the service of the latter, first as an assistant in the revision of the statute-book, and later on the more personal service of maintaining the policy of the Elector, that of defending the integrity of the German Empire against the intrigues of France, Turkey and Russia, by his pen.

[70] This probably refers to the time when his work on the statute-book was concluded, and Leibniz was preparing to look for employment elsewhere.

[71] This is worthy of remark, seeing that Leibniz had attempted to explain gravity in the *Hypothesis physica nova* by means of his concept of an ether. The conversation with Huygens had results that will be seen later in a manuscript (§ III below) where Leibniz obtains quadratures "*ex Centrobarycis*." It also probably had a great deal to do with Leibniz's concept of a "moment."

[72] The use of the word *veterno*—which I have translated "lethargy" as being the nearest equivalent to the fundamental meaning, the sluggishness of old age—coupled with his remark that he was in no mind to enter fully into these more profound parts of mathematics, sheds a light upon the reason why he had so far done no geometry. Also the last words of the sentence give the stimulus that made him cast off this lethargy; namely, shame that he should appear to be ignorant of the matter. This would seem to be one of the great characteristics of Leibniz, and might account for much, when we come to consider the charges that are made against him.

[73] We have here a parallel (or a precedent) for my suggestion that Leibniz was mentally confusing Barrow and Pascal as the source of his inspiration for the characteristic triangle. For here, without any doubt whatever, is a like confusion. What Pell told him was that his theorems on numbers occurred in a book by Mouton entitled *De diametris apparentibus Solis et Lunae* (published in 1670). Leibniz, to defend himself from a charge of plagiarism, made haste to borrow a copy from Oldenburg and found to his relief that not only had Mouton got his results by a different method, but that his own were more general. The words in italics are interesting.

Of course these words are not italicized by Gerhardt, from whom this account has been taken (G. 1848, p. 19); nor does he remark on Leibniz's lapse of memory in this instance. Further there is no mention made of it in connection with the *Historia*, i. e., in G. 1846. Is it that Gerhardt, as counsel for the defense, is afraid of spoiling the credibility of his witness by proving that part of his evidence is unreliable? Or did he not become aware of the error till afterward? See Cantor, III, p. 76.

[74] An instance is referred to on p. 85 of De Morgan's *Newton*, showing the sort of thing that was done by the committee. This however is not connected with a letter to Oldenburg, but to Collins. It may be taken as a straw that shows the way the wind blew.

[75] Observe that nothing has been said of the fact that Leibniz had purchased a Barrow and took it back with him to Paris.

[76] Cf. the remark in the postscript to Bernoulli's letter, where Leibniz says that the work of Descartes, looked at at about the same time as Clavius, that is, while he was still a youth, "seemed to be more intricate."

[77] The *libellus* referred to would seem to be the work on the cycloid, written by Pascal in the form of letters from one Amos Dettonville to one M. de Carcavi.

[78] This theorem is given, and proved by the method of indivisibles, as

Theorem I, of Lecture XII in Barrow's *Lectiones Geometricae*; and Theorem II is simply a corollary, in which it is remarked:

"Hence the surfaces of the sphere, both the spheroids, and the conoids receive measurement...."

The proof of these two theorems is given at the end of the section as a supplement. See also Note 46, for its significance.

[79] The whole context here affords suggestive corroboration in favor of the remarks made in Note 31 on the use of the word "moment," though the connection with the determination of the center of gravity is here overshadowed by its connection with the surface formed by the rotation of an arc about an axis.

[80] The figure given is exactly that given by Gerhardt, with the unimportant exception that, for convenience in printing, I have used U instead of Gerhardt's Θ, a V instead of his ה (a Hebrew T), and a Q for his Π. I take it, of course, that Gerhardt's diagram is an exact transcript of Leibniz's, and it is interesting to remark that Leibniz seems to be endeavoring to use T's for all points on the tangent, and P's for points on the normal, or perpendicular, as it is rendered in the Latin.

This diagram should be compared with that in the "postscript" written nine or ten years before. Note the complicated diagram that is given here. and the introduction of the secant that is ultimately the tangent, which does not appear in the first figure. From what follows, this is evidently done in order to introduce the further remarks on the similar triangles. It adds to the confusion when an effort is made to determine the dates at which the several parts were made out. For instance, the remark that finite triangles can be found similar to the characteristic triangle probably belongs approximately to the date of his reply to the assertions of Nieuwentiit, which will be referred to later.

[81] The notation introduced in the lettering should be remarked. His early manuscripts follow the usual method of the time in denoting different positions of a variable line by the same letter, as in Wallis and Barrow, though even then he is more consistent than either of the latter. He soon perceives the inconvenience of this method, though as a means of generalizing theorems it has certain advantages. We therefore find the notation C, (C), ((C)), for three consecutive points on a curve, as occurs in a manuscript dated (or it should be) 1675. This notation he is still using in 1703; but in 1714, he employs a subscript prefix. This is all part and parcel with his usual desire to standardize and simplify notations.

[82] This sentence conclusively proves that Leibniz's use of the moment was for the purposes of quadrature of surfaces of rotation.

[83] "From these results"—which I have suggested he got from Barrow—"our young friend wrote down a large collection of theorems." These theorems Leibniz probably refers to when he says that he found them all to have been anticipated by Barrow, "when his Lectures appeared." I suggest that the "results" were all that he got from Barrow on his first reading, and that the "collection of theorems" were found to have been given in Barrow when Leibniz referred to the book again, after his geometrical knowledge was improved so far that he could appreciate it.

[84] The use of the first person is due to me. The original is impersonal, but is evidently intended by Leibniz to be taken as a remark of the writer, "the friend who knew all about it." The distinction is marked better by the use of the first personal pronoun than in any other way.

[85] Query, all except Leibniz, the Bernoullis, and one or two others.

[86] Tetragonism = quadrature; the arithmetical tetragonism is therefore Leibniz's value for π as an infinite series, namely,

"The area of a circle, of which the square on the diameter is equal to unity, is given by the series

$$\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11} + \text{etc.}$$

[87] This is clearly original as far as Leibniz is concerned; but the consideration of a polar diagram is to be found in many places in Barrow. Barrow however forms the polar differential triangle, as at the present time, and does not use the rectangular coordinate differential triangle with a polar figure; nor does Wallis. We see therefore that Leibniz, as soon as ever he follows his own original line of thinking, immediately produces something good.

[88] This is evidently a misprint; it is however curious that it is repeated in the second line of the next paragraph. Probably, therefore, it is a misreading due to Gerhardt, who mistakes AZ for the letters XZ, as they ought to be; and has either not verified them from the diagram, or has refrained from making any alteration.

[89] The symbol $\smile$ is here to be read as "and then along the arc to."

[90] Probably refers to Leibniz's work on curvature, osculating circles, and evolutes, as given in the *Acta Eruditorum* for 1686, 1692, 1694. It is to be noted that with Leibniz and his followers the term evolute has its present meaning, and as such was first considered by Huygens in connection with the cycloid and the pendulum. It signified something totally different in the work of Barrow, Wallis and Gregory. With them, if the feet of the ordinates of a curve are, as it were, all bunched together in a point, so as to become the *radii vectores* of another curve, without rupturing the curve more than to alter its curvature (the area being thus halved), then the first curve was called the evolute of the second and the second the involute of the first. See Barrow's *Lectiones Geometricae*, Lecture XII, App. III, Prob. 9, and Wallis's *Arithmetica Infinitorum*, where it is shown that the evolute, in this sense, of a parabola is a spiral of Archimedes.

[91] The colon is used as a sign of division, and the comma has the significance of a bracket for all that follows. It is curious to notice that Leibniz still adheres to the use of xx for x^2, while he uses the index notation for all the higher powers, just as Barrow did; also, that the bracket is used under the sign for a square root, and that too in addition to the vinculum. For an easy geometrical prof of the relation $x = 2z^2/(1+z^2)$, see Note 94.

[92] See Cantor, III, pp. 78-81. Also note the introduction of what is now a standard substitution in integration for the purpose of rationalization.

[93] This term represents what is now generally known as the method of inversion of series. Thus, if we are given

$$x = y + ay^2 + by^3 + cy^4 + \text{etc.},$$

where x and y are small, then $y = x$ is a first approximation; hence since $y = x - ay^2 - by^3 - cy^4 - \text{etc.}$, we have as a second approximation

$$y = x - ax^2;$$

substituting this in the term containing y^2, and the first approximation, $y = x$, in the term containing y^3, we have

$$y = x - a(x - ax^2)^2 - bx^3 = x - ax^2 + (2a^2 - b)x^3,$$

as a third approximation; and so on.

[94] The relation $x = 2z^2/(1+z^2)$ can be easily proved geometrically for the circle; hence, by using the orthogonal projection theorem, Leibniz's result for the central conic can be immediately derived.

Thus suppose that, in the diagrams below, AC is taken to be unity, AU $= z$ and AX $= x$.

Then, in either figure, since the Δs BYX, CUA are similar,

$$AX : XB = AX.XB : XB^2 = XY^2 : XB^2 = AU^2 : CA^2;$$

hence, for the circle, we have

$$AX : AB = AU^2 : AC^2 + AU^2, \text{ or } x = 2z^2/(1+z^2);$$

and similarly for the rectangular hyperbola

$$AX : AB = AU^2 : AC^2 - AU^2, \text{ or } x = 2z^2/(1-z^2).$$

Applying all the x's to the tangent at A, we have (by division and integration of the right-hand side, term by term, in the same way as Mercator)

$$\text{area AUMA} = 2(z^3/3 \mp z^5/5 + z^7/7 \mp \text{etc.})$$

Now, since the triangles UAC, YXB are similar, UA.XB = AC.XY; hence $2\triangle AYC = 2UA.AC \mp UA.AX = 2UA.AC \mp AUMA \mp 2\,\text{seg.}\,AYA$,

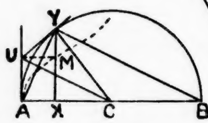


Fig. G.

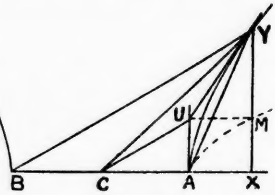


Fig. H.

for Leibniz has shown that AXMA = 2 seg. AYA; hence it follows immediately that

$$\text{sector ACYA} = z \mp z^3/3 + z^5/5 \mp \text{etc.}$$

If now, keeping the vertical axis equal to unity, the transverse axis is made equal to a, Leibniz's general theorem follows at once from the orthogonal projection relation.

Note that z is, from the nature of the diagrams, less than 1.

[95] Wallis's expression for π as an infinite product, given in the *Arithmetica* (or Brouncker's derived expression in the form of an infinite continued fraction), or the argument used by Wallis in his work, could not possibly be taken as a proof that π could not be expressed in recognized numbers.

[96] The letter that is missing would no doubt have been given, in the event of the *Historia* being published. According to Gerhardt it is to be found in *Ch. Hugenii....exercitationes,* ed. Uylenbroeck, Vol. I, p. 6, under date Nov. 7, 1674.

[97] Collins wrote to Gregory in Dec. 1670, telling him of Newton's series for a sine, etc.; Gregory replied to Collins in Feb. 1671, giving him three series for the arc, tangent and secant; these were probably the outcome of his work on *Vera Circuli* (1667).

[98] By Mercator; query, also an allusion to Brouncker's article in the *Phil. Trans.,* 1668.

[99] Quite conclusive; no other argument seems required.

[100] This date, April 12, 1675, is important; it marks the time when Leibniz first began to speak of geometry in his correspondence with Oldenburg, as he says below.

[101] Newton obtained the series for arcs in x from the relation $\dot{a} : \dot{x} = 1 : \sqrt{(1-x^2)}$, by expansion and integration, and then the series for the sine by the "extraction of roots." See Note 93, and, for Newton's own modification, Cantor, III, p. 73.

[102] It would appear from this that Leibniz could differentiate the trigonometrical functions. Professor Love, on the authority of Cantor, ascribes them to Cotes; but I have shown in an article in *The Monist* for April, 1916, that Barrow had explicitly differentiated the tangent and that his figures could be used for all the other ratios.

[103] Probably only to test Leibniz's knowledge.

[104] Gerhardt states that in the first draft of the *Historia*, Leibniz had bordered the Harmonic Triangle, as given here, with a set of fractions, each equal to 1/1, so as to more exactly correspond with the Arithmetical Triangle.

[105] The sign here used appears to be an invention of Leibniz to denote an identity, such as is denoted by $\equiv$ at present.

[106] This, and other formulas of the same kind, had been given by Wallis in connection with the formulas for the sums of the figurate numbers. Wallis called these latter sums the "characters" of the series.

[107] This sentence, in that it breaks the sense from the preceding sentence to the one that follows, would appear to be an interpolated note.

[108] There is an unimportant error here. The first value of x evidently should be 0, and not 1.

[109] Why not? Newton's dotted letters still form the best notation for a certain type of problem, those which involve equations of motion in which the independent variable is the time, such as central orbits. Probably Leibniz would class the suffix notation as a variation of his own, but the D-operator eclipses them all. For beginners, whether scholastic or historically such (like the mathematicians that Barrow, Leibniz and Newton were endeavoring to teach), the separate letter notation has most to recommend it on the score of ease of comprehension; we find it even now used in partial differential equations.

[110] Leibniz does not give us an opportunity of seeing how he would have written the equivalent of $dxdxdx$; whether as dx^3 or $\overline{dx^3}$ or $(dx)^3$.

[111] *Ductus* and *ungulae* have already been explained in Notes 28, 29; *cuneus* denotes a wedge-shaped solid, cf. "cuneiform."

[112] This only proves the proportionality, enabling Leibniz to convert the equation $2\int dy/y = 3\int dx/x$ into $2 \log y = 3 \log x$. It will hardly suffice as it stands to enable him to deal with such an equation as $2\int dy/y = 3\int x\, dx$; and it is to be noted that Leibniz does not notice at all the constant of integration. Although Barrow has differentiated (and therefore also has the inverse integral theorems corresponding thereto) both a logarithm and an exponential in Lecture XII, App. III, Prob. 3, 4, yet these problems are in such an ambiguous form that it may be doubted whether Barrow was himself quite clear on what he had obtained. Hence this clear statement of Leibniz must be considered as a great advance on Barrow.

[113] Almost seems to read as a counter-charge against Newton of stealing Leibniz's calculus. Note the tardy acknowledgement that Barrow has previously done of all that Newton has given.

[114] The whole effect that this *Historia* produces in my mind is that the entire thing is calculated to the same end as the *Commercium Epistolicum*. The pity of it is that Leibniz could have told such a straightforward tale, if events had been related in strict *chronological* order, without any interpolations of results that were derived, or notation that was perfected, later. A tale so told would have proved once and for all how baseless were the accusations of the *Commercium*, and largely explained his denial of any obligations to Barrow.

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CRITICISMS AND DISCUSSIONS.

CURRENT PERIODICALS.

The number of the *Revue de métaphysique et de morale* for January, 1916, is wholly devoted to the commemoration of Malebranche, whose death took place on October 13, 1715. Maurice Blondel writes on the anti-Cartesianism of Malebranche, Emile Boutroux on the intellectualism of Malebranche, Pierre Duhem on the optical work of Malebranche, R. Thamin on Malebranche's *Traité de morale,* E. van Biéma on how Malebranche conceived psychology, and Victor Delbos on Malebranche and Maine de Biran; while Desiré Roustan puts in a plea for an edition of the collected works of Malebranche.

* * *

Among the especially noteworthy articles in the *Bulletin of the American Mathematical Society* for 1916 are reviews which are wonderful examples of research, by Prof. R. C. Archibald of books on the life and work of Napier, and of mathematical quotations (January number), and of Goldenring's history of the construction of a regular polygon of seventeen sides (February number); Dr. R. L. Moore's article on a non-metrical pseudo-Archimedean axiom (February number); and Prof. E. J. Wilczynski's address on the historical development and the future prospects of the differential geometry of plane curves, in which a precise and profound delimitation of the subject-matter of differential geometry is given.

* * *

There are three papers of particular interest to the readers of *The Monist* in the number of the *Transactions of the American Mathematical Society* for January, 1916: Prof. W. F. Osgood sets at rest some interesting questions in the theory of analytic functions of several complex variables by means of simple examples;

Profs. E. B. Van Vleck and F. H. Doubler study Theta functions as defined by functional equations; and Dr. B. A. Bernstain, starting from *class* and *operation* as primitive ideas, succeeds in reducing to four the number of postulates necessary for Boole's algebra of logic.

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In the number of *Scientia* for February 1916, the Abbé Th. Moreux discusses the problem of the novae—stars which appear suddenly at certain periods in the heavens—and the constitution of the universe. Fillippo Bottazzi gives the second part of his article on the fundamental physiological activities; this part is on muscular activity. Annie S. D. Maunder (Mrs. Walter Maunder) deduces some interesting things about prehistoric Iranian migrations from passages in sacred books of Persia—the *Vendidad* and the *Tir Yasht.* Charles Gide writes on the expenditures of the belligerent nations and their economic consequences; and Achille Loria writes on the probable social and economic consequences of the war. Besides this there are reviews of books and periodicals, and French translations of articles in Italian and English.

In *Scientia* for March, 1916, C. G. Abbot writes on the sun as regards its composition and state as transmitter and receiver of energy. E. Bouty gives the first part of an article on the kinetic theory of gases. This part is devoted to the foundations, and it is interesting to notice that the author says that in a kinetic and therefore mechanistic theory we must consider, besides visible motions, *hypothetical and invisible motions.* Louis Matruchot writes on the light thrown on the problem of cancer by vegetable pathology, cancers having been discovered in vegetables. Otto Jespersen of Copenhagen gives some reflections of a Dane on the war; and Camillo Supino of Pavia writes on the economic sources of the war. The number is completed by book reviews and a review of periodicals.

In *Scientia* for April, 1916, Aldo Mieli writes on the pneumatic period of chemistry: the study of gases from the time of Robert Boyle to that of Lavoisier. E. Bouty gives an account of the development and difficulties of the kinetic theory of gases; the question of thermal radiation will be treated in another article as this subject is a great difficulty in the way of the kinetic theory. Etienne Rabaut writes on embryonic phenomena and phylogenesis. J. Holland Rose of Cambridge, England, discusses the future of

Europe; and C. A. Reuterskiöld of Upsala in Sweden indicates what he thinks should be the chief lines of international law after the war. There is a general review of the problems of the fable with special reference to Hindu literature, by A. M. Pizzagalli. There are also reviews of books and periodicals.

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The number of *Scientia* for June 1916 opens with a suggestive article by Professor Pincherle on "Intuition and the Calculus of Probabilities." The word "probability" or "chance" is meaningless to the man from whom no causes are hidden. The definition of "probability" implies the principle of equivalence of causes, and this symmetrical principle implies the absence of any cause which is even in the smallest degree a dominating cause. The simpler cases with which the calculus of probabilty deals are those in which the number of possible causes is finite. Things become more complicated when that number is no longer finite, or when the possible causes form a continuum, for the elementary definition of probability must now be generalized. The author proceeds to show that there is more than a simple agreement between the data of intuition and the theoretical results of the calculus of probabilities. By the elaboration of a few principles of extreme simplicity, the calculus substitutes, as it were, for these data, propositions frankly deductive in their character. Thus intuition comes into play—first in the preliminary exploration of a question, secondly, in helping us to foresee results, and finally, in detecting from this or that result the weak spot at which the assault of scientific criticism may be most effective. Prof. W. M. Bayliss deals with "Surface Phenomena in Living Structures." He is inclined to think that it may be safely said that the peculiarities of the so-called "vital" phenomena are due to the fact that they constitute manifestations of exchange of energy between the phases of a heterogeneous system. Special degrees of activity may be detected during the transformation of energy, e. g., electric phenomena during the oxidation of phosphorus or benzaldehyde. Life is an incessant change, or a continuous transfer of energy, and a system in a state of statical equilibrium is equivalent to death. In "After the War," Ettore Ciccotti foresees that the causes of conflict between nations are too deeply rooted to be eliminated by this war, whatever may be its result. The hegemony of the money market will be tranferred from the Old World to the New. The experiences of the last two years

will force every nation to undertake an exhaustive examination of its natural resources, and all energies will be devoted to the development of productive forces, and to organization for the purpose of unifying, multiplying, and rendering immediately available the energies of the state. Social justice, emancipation from class domination, and the recognition of peace as the universal goal of humanity will be the chief articles in the creed of the new international socialist party. And finally we may see, for the next generation or so, a humanity penetrated by the most poignant of pessimisms.

The July number opens with a paper by Antonio Favaro on the "Effect of the Condemnation of Galileo upon the Progress of Science." One of the most serious consequences was the difficulty found by men like Descartes in the full expression of their thought. Rome was powerless to check the innermost thoughts of men, but she could and did use her powers of intimidation to such effect that what should have been the philosophy of the age was either directly checked, made but a timid advance, or was diverted from its natural channels. All that was new or out of the common rut came under suspicion. All the protestations and submissions of Descartes could not prevent his works being placed on the Index. At the nod of a Richelieu the Sorbonne returned to a sun revolving round the earth.—The study of the phenomena of cathodic bombardments, set forth in his article "The Colloids and Projections from Cathodes," has led Professor Houllevigue to the conclusion that the projectiles launched by an electrode of silver are of the same order of magnitude as the granules of colloidal silver deposited in the Bredig process. Experiments carried on for several years have brought him to the belief that it will clarify our ideas if we cease for the moment the study of colloids from the point of view of a solid or liquid state, and consider what takes place in the gaseous colloidal medium which surrounds the cathode in activity in a vacuum tube. This view he throws out with some reserve; but, as he reminds us, even if an hypothesis proves to be unfounded, it may still play its part in the progress of science by the experiments to which it leads.—Professor Lalande contributes a subtly conceived little paper on the "Relations between Logic and Psychology." The progress of logical intelligibility is marked by the discovery of resemblances in given differences. The ideal of scientific success is the absorption of facts *sui generis* in a wider formula common to

them all. We may not reach the why and the wherefore of the world by means of the logical norm, but the rich diversity of the universe provides for that norm, as it were, the fuel for the fire.—The "Reparation of the Waste of War," and the "Principal Economic Consequences of the Interruption of International Exchanges" form the texts for two articles by Mr. W. R. Scott and F. Virgilii respectively. Dr. Jankelevitch reviews the series of articles that have appeared in *Nature* and *Science Progress* dealing with the organization of science, its relations to the state, and the proper payment of scientific men.

In the August *Scientia* J. L. Heiberg discusses the role of Archimedes in the development of the exact sciences. The author describes the probable equipment with which Archimedes began his mathematical labors. His mastery of the weapons of his age in the attack on the theory of the conic sections, and their application to the solution of problems of a higher order, was considerable enough to win for Apollonius in later days the title of "plagiarist." The spiral of Archimedes was a magnificent geometrical effort which was later utilized in important investigations on the surface of the cylinder and sphere. The *Arenarius* reminds us of his success in dealing with large numbers. The influence of the great Greek upon succeeding ages is then carefully traced. The treatise on mechanical method, discovered but a decade ago,[1] would have greatly simplified the work of Kepler and Cavalieri had it been in their hands.—The "Hydrology of the Carso" of Istria, Carniola and Trieste, forms the subject of a most interesting geological paper by Prof. Luigi De Marchi.—A paper by Prof. L. Vialleton on the biogenetic law is based upon the precocity of the appearance of different types of the same group in the paleontological development. There is an undoubted parallelism between paleontological and ontogenetic development. Both issue at an early stage in well-defined and often divergent forms between which are no intermediaries. The anterior limb of the lemur could never be transformed into the wing of the bat, because its construction enables it to act in a vertical or nearly vertical plane, and never in the horizontal plane as in the case of the wing. There is little doubt that Cuvier's correlation law will play an important part in the explanation of the morphological puzzles that have yet to be unravelled.—Messrs. J. B.

[1] *Geometrical Solutions Derived from Mechanics.* Discovered and translated by Professor Heiberg. English edition published by Open Court Publishing Company, 1909.

Clark and E. Catellani treat respectively of the economic dynamics of war and the conditions under which peace may be secured and further outbreaks of war prevented.

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The number of *Science Progress* for April, 1916, contains papers by James Johnstone on the mathematical theory of organic variability, by David Fraser Harris on the specific characteristics of vitality, by C. Mansell Moullin on the natural history of tumors, and by Joseph Offord on the knowledge of the ancients regarding the propagation of disease by flies and rodents; and the third part of the investigations by Sir Ronald Ross on the solution of equations by operative division. Besides this there are very many reviews of books, notes, correspondence, and the usual long quarterly reports on the recent advances made in the various branches of science.

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With the July number of *Science Progress* a new volume begins—the eleventh—and an extension of purview is shown by the addition of "and Affairs" to the old title, "A Quarterly Review of Scientific Thought and Work." Articles no longer are awarded the bulk of the space at the disposal of the Editor. Just over three quarters of the number are given to notes, essays, reviews and to the very valuable pages entitled "Recent Advances in Science," now running to 50 pages or so. Mr. Bradford's "Historical Sketch of the Chemistry of Rubber" closes with an expression of confidence that before very long we shall have a synthetic rubber on the market. Mr. Friend deals with the "Bionomics of English Oligochaeta," Part ii—a most useful piece of (unpaid) work, in which stress is laid on the benignant role of Pachydrilids in the economy of nature. "A Biologist" in "The Pollution of the Sea" has an opportunity, of which he cordially avails himself, of exposing the mischiefs inherent in lawyer-made law upon matters dealing with the realities of life. And Mr. Reid Moir is at home in "Flint Fracture and Flint Implements," giving an account of experiments devised to distinguish between human and natural flaking. Among the essay-reviews is a long and interesting account of a great medical reformer—John Shaw Billings, "a man who was unique in the history of his profession." Φ

IDO AND ENGLISH.

As a believer in the feasibility, practicability and necessity of an international language, and, after investigating about sixty such projects, finding Ido by far the best and most perfect, I was greatly pleased to see in *The Monist* of January, 1916, a short grammar of this language. Incidentally allow me to mention that there are some errors in the exposition in *The Monist,* the most important of which is on page 149, line 3, where instead of "*qua,* who (masculine), *qui,* who (feminine), *quo,* what (neuter)," it ought to be: "*qua,* who or which (singular), *qui,* who or which (plural), *quo,* what."

But my object in writing to you is principally to argue against the following article in *The Monist*: "English as a Universal Language," by Albon P. Man, Jr. He thinks that a simplification of English spelling would make the English language fit to become "the universal language." This is not a new proposition, but the fact that English is now the most widely diffused language does not prove that it is fit to become the "universal," or as I prefer calling it, the "international" language, for the promoters of this idea do not intend that it should supplant the other national languages, but that it should be for all the "second" language, next to their mother tongue.

It is universally acknowledged that English, though comparatively easy in its grammar, compared to most other natural languages, is extremely difficult, not only in its orthography, but in its pronunciation and so-called accent. A foreigner may be able to speak English correctly, but almost at the first word one will be able to notice that he is a foreigner. Besides, in order to speak English correctly a foreigner needs long and arduous study, unless he happens to live in an English-speaking country.

Now if English (or any other national language) should be selected as the "second" language for all, those whose "first" (or mother-) language it is, would have an immense advantage, an advantage which other nations would hardly be willing to concede to it. And even then those to the manner born would be able to speak it more fluently, with less mental exertion and without a foreign accent.

But leaving this point aside, does any one suppose that after this war the most important civilized nations will accept English

(or any other national language) as an international medium? And without such acceptance no language, natural or artificial, can become that medium.

A simplification of English spelling would not make English appreciably easier for foreigners; it would make it easier for English and American children who know the language already, but not for others. Besides, even the reformed spelling gives absolutely no clue how a word should be pronounced, unless one knows the word already. To take one or two examples from Mr. Man's own letter: Why should "been" and "in" be pronounced with a short *i* and spelt differently? Who can guess that in "sho" and "to," though written with the same vowel, that vowel is pronounced differently, etc., etc.

All this shows that only a "neutral" language, which also in its grammar, spelling and word-construction is easy, can ever hope to be accepted as "the international language."

C. T. Strauss.

Leipsic, Germany.

BOOK REVIEWS AND NOTES.

Contributions to the Founding of the Theory of Transfinite Numbers. By *Georg Cantor*. Translated and provided with an Introduction by *Philip E. B. Jourdain*. Chicago and London: Open Court Publishing Company. Pages, 212, Price, $1.25.

Everybody knows and constantly uses the whole numbers, 1, 2, 3, and so on; and uses the word "infinite" for something which, like the above series of numbers, has no end. In fact, however large a number is, we can always think of a still larger one, and thus we never get to an end of the above series. But the great German mathematician Georg Cantor, who is still living at Halle, first saw about 1870 that in certain branches of mathematics we must contemplate a new series of numbers each of which is greater than any of the above finite numbers, and thus has a place *after all* the finite numbers; just as in the spectrum a shade of red has a place after all the innumerable shades of orange though we cannot say that there is a last shade of orange. Cantor spent years in getting himself and others accustomed to the strange idea of infinite or "transfinite" numbers, which, though each consisted of an unending set of units, could be thought of as complete wholes much as "all the points in the line AB" denotes an infinite set and can yet be treated as a completed whole. With this end in view Cantor studied deeply the arguments of philosophers, theologians and mathematicians about the infinite. At last, in 1895 and 1897, he succeeded in putting the results of nearly thirty years of work into a logical form which any intelligent person will not find very hard to understand. It is these famous essays that are here translated. In the introduction Mr. Jourdain has shown in detail how the new ideas grew from the work of Cantor's predecessors and in Cantor's own mind, and how these ideas must now be studied and used by *all philosophers, theologians, logicians, those interested in the foundations of the science of number and all mathematics,* and those who think about the ultimate constitution of space and matter, besides all mathematicians. This book appeals to any one who wants to understand one of the main things that has revolutionized many of the methods and problems and applications of modern mathematics and philosophy of mathematics and philosophy in general, and feels sympathy with those who want to know what numbers and fractions and space and matter are.

Why should mathematics interest everybody? Mere calculation is not interesting except to a few people. But even letting the mind rest on great and firm eternal truths is enchanting; living and working to find out more about them is absorbing. *Mathematics is one of the few paths to truth, and*

the search for truth is the religion of all thinking men and women nowadays. Mathematics is one of the most living of studies when treated historically so that we can follow the birth and development of great ideas. Thinking teachers know how attractive and indispensable it is to introduce students to new ideas and the truths they mirror, slowly and, if possible, as the actual discoverers were introduced to them. ϕ

NAPIER TERCENTENARY MEMORIAL VOLUME. Edited by *Cargill Gilston Knott.* Published for the Royal Society of Edinburgh by Longmans, Green and Co., London and New York, 1915. Pp. xii, 441. Price $7 net or 21s. net.

This magnificent volume contains the addresses and essays communicated to the international congress held at Edinburgh in July, 1914, in celebration of the tercentenary of the first publication of John Napier's system of logarithms. It is superbly printed and bound, contains a frontispiece in color from the well-known portrait of Napier in the University of Edinburgh and has several other plates. This congress, of which a full account is given by Dr. Knott, was the last international congress of any kind held before the European war broke out; and there is a certain melancholy interest in glancing through this volume and seeing contributions of great value not only from Great Britain but also from America, France, Germany, Italy, and even Turkey. The communications fall into two groups. Some treat of the life and work of Napier, and some with subsequent developments of the logarithmic idea and contain valuable additions to our means of calculation. But the greatest interest, perhaps, will center in the contributions of the first group, and of these the most striking is the inaugural address by Lord Moulton, in which an attempt is made to reconstruct the gradual evolution of Napier's great discovery. Most of us know that Lord Moulton, in his career at the Bar, had great experience in the study of inventions, and this address of his is one of the most important contributions to the history of mathematics that has been made in recent years. Indeed the whole volume is quite indispensable for the future historian of mathematics. We may mention that Prof. F. Cajori shows how the history of the subject has been mangled by authoritative historians of the past, and that there are also notable contributions made by Dr. J. W. L. Glaisher, Prof. D. E. Smith, Prof. G. A. Gibson, and many others. Finally it must be mentioned that a copy of the rare work of Bürgi was lent to the congress by the town library of Danzig and it is fully described in this volume. Φ

A COURSE OF MODERN ANALYSIS: An Introduction to the General Theory of Infinite Processes and of Analytic Functions; with an Account of the Principal Transcendental Functions. By *E. T. Whittaker* and *G. N. Watson.* Second edition, completely revised. Pp. vi, 560. Cambridge (England): University Press, 1915. 18s. net.

The first edition (by Professor Whittaker alone) of this work was published in 1902, and in the preparation of the second edition Professor Whittaker has been most ably helped by Mr. Watson. To Mr. Watson the new chapters on Riemann Integration, Integral Equations, and the Riemann Zeta-Function

are practically wholly due. Part II ("The Transcendental Functions") is, as we should expect, most admirably done; but, since the subject-matter is exclusively technical, the philosopher and logician will turn with more interest to those chapters in Part I ("The Processes of Analysis") in which more fundamental subjects are discussed. It is a most pleasing fact that the treatment of irrational numbers (pp. 4-6), the theory of convergence (pp. 11-40), and the proof of the theorem of Cauchy and Goursat on complex integration (pp. 53-54, 84-87) by the help of the "modified Heine-Borel theorem," are so well done in this new edition. The theorem attributed to Bolzano (p. 13) was not really proved by Bolzano. Bolzano used, in 1817 and not in 1851 as stated, the same *process* which afterwards, in the hands of Weierstrass, led to an exact proof. The exact proof of the condition mentioned on page 14 is also due to Weierstrass and not to Cauchy. The book is a thoroughly good one, and will be of great value in English and American universities. Φ

FUNDAMENTAL CONCEPTIONS OF MODERN MATHEMATICS. By *Robert P. Richardson* and *Edward H. Landis*. Chicago: The Open Court Publishing Co., 1915. Cloth, $1.25 net.

This work deals, not with the technicalities of mathematics or with its applications as an art, but with a basis for its scientific development. In considering mathematics as a science rather than as an art two points of view may be taken. With the first, that of pure formalism, the scope of the investigation hardly goes beyond symbols and the laws of their combination, little heed being paid to what these symbols represent. The prevailing tendency is to look at mathematical science in just this aspect, but the authors of the present work, preferring a broader outlook, have chosen the second viewpoint where attention is focussed upon the subject matter of the science, the form in which this is symbolically expressed being regarded as of minor importance. They are not content to rest satisfied with a science of symbols, but inquire into the realities underlying mathematical formulas. Naturally a primary object of the quest is to furnish a clear and precise explanation of the nature of the various types of quantities represented by the symbols of mathematics. This cannot be satisfactorily done by merely giving a résumé of doctrines already current, for the field of inquiry here was largely virgin soil and much original work was necessary to attain a theory that accorded with mathematical practice. The account given of quantities and their classification goes into the matter with great detail, and has in view not merely the quantities of ordinary algebra but likewise those of quaternions and of all other branches of algebraic science. Equally thorough is the consideration given to the constitution of variables and the essential characteristics of a functional relation between variables. Besides these three main topics the discussion takes up a number of other questions, minor ones relatively speaking but of no small importance to the theory of mathematics. The book, which has as subtitle *Variables and Quantities with a Discussion of the General Conception of Functional Relation*, is the first of a series projected to cover all the fundamental conceptions of modern mathematics, but it is a complete work in itself, and the questions that come within its scope are by far the most fundamental of all arising in mathematical science.

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